Test-theory Methodology in Physics

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Contents

Abstract

Test theories are mathematical constructions with a methodological purpose. They enable experimental or other observational evidence definitively to select a physical theory from out of a class of alternatives—a formal deductive method of testing which is (as I will show) distinct from the traditional hypothetico-deductive one. I believe that the study here of test-theory methodology is important for philosophers of science if they are properly to understand (i) *methods* of theory appraisal in physics, (ii) certain key conceptual issues in the *foundations* of physics, and (iii) more generally, the growth and structure of physical theory (*history*). I believe that this study is important also for physicists who desire a clearer understanding of the power, limitations, and conceptual presuppositions of the test-theory method.

(i) *Methods.* I show how test theories establish relations between physical theory and phenomena which are at once stronger and more systematic than relations established by the hypothetico-deductive method. I use this result to defend some positivist methodological doctrines which philosophers today generally reject. I also use the result to criticise certain post-positivist doctrines which are meant to replace the very positivist doctrines I aim to defend. I compare some philosophical accounts of empirical confirmation with test-theory methodology in order to illuminate some features of the test-theory method and criticise standard philosophical accounts of confirmation. In this discussion I explore limitations of the test-theory method and identify general conditions which both knowledge and the world must satisfy if the method is to work.

(ii) *Foundations.* I address some key issues in the foundations of physics, issues concerning the nature of space, time and causation. I discuss how test-theory methodology reveals the extent to which the very foundations of physics are empirically determinable. This extent is limited I argue: my discussion points to the furthest application but also to some notable limitations of the test-theory approach.

(iii) *History.* I argue that Newton pioneered the use of test-theory constructions in theoretical physics, and I show how twentieth century physicists have brought the test-theory method to a high level of sophistication. I argue both that the test-theory idea makes possible a coherent view of the history of physics, and that the history of physics deepens our appreciation of the power of the test-theory idea. There is unity, I argue, at the level of test-theory methodology across the seemingly disparate historical phases of physical inquiry (classical versus modern). There is power, I maintain, in a method which can fruitfully be applied in seemingly diverse conceptual contexts (Newtonian versus Einsteinian).

Preface

Test-theory constructions allow empirical evidence to select a physical theory by displaying that theory as falling within a mathematically defined, "parameterised", family of alternatives. The parameter in question is appropriately chosen so that (granting some background assumptions which have been clearly delimited and are typically unproblematic) its value may be definitively "measured" by specific experimental or other observational evidence. The empirical selection of physical theories from a class of alternatives is one very important function of test-theory constructions. In this dissertation I will also draw attention to other key functions, including (1) the way in which test theories provide a means of classifying physical theories, and thereby a measure of the "closeness" of one physical theory to another, and (2) the use of test theories as tools for discovery in theoretical physics.

Test-theory methodology is the invention of physicists not philosophers. Even the name 'test theory' has been coined by physicists. The development of test-theory methodology illustrates how methods of empirical discovery in science are themselves discovered empirically. Test theories have delivered to theoretical physicists the kind of theories they seek—theories of great explanatory power. Physicists themselves endorse the test-theory method on the quasi-empirical cum pragmatic basis that it works, that it furthers science's aim to provide explanations of natural phenomena. These facts about theoretical physics make clear how methodologically reflective physicists really are. Thus, physicists challenge philosophers of science to attend not only to tacit, or implicit, aspects of the practice of physics, but also to patterns in that practice which physicists themselves have made explicit. As far as I am aware I am the first philosopher of science to respond to this challenge by adopting the terminology that physicists themselves use.

Physicists have been using test theories for some time now. Recent studies in the history and philosophy of science have championed accounts of empirical confirmation involving "demonstrative" and "eliminative" induction, "deduction from phenomena", and "bootstrap" inferences. I will show how these accounts support the same kind of relations between theory and evidence that are established by means of test theories. I will argue that both test-theory methodology and bootstrap methodology, in particular, make credible certain methodological tenets of logical positivism which many philosophers today reject.

Besides demonstrating the importance of test theories for philosophers of science I wish also in the dissertation to help physicists better appreciate the test-theory method. I have stated already my belief that physicists today understand well the test-theory method which they have developed. Yet I also believe that a historically informed philosophical study of this method, such as I undertake in this dissertation, can lead to even greater understanding, and possibly even to more significant application, of it. Physicists today emphasise the method's use in testing physical theories—hence the name 'test theory'. But one use of the test-theory method which its contemporary practitioners do not stress is its potential as a tool for theoretical discovery. I will argue, on the basis of its empirical credentials, its applicability in a wide range of conceptual contexts, and also on the basis of its unifying power, that the test-theory method is an appropriate method to discover theories which further unify the fundamental physical interactions. I will endeavour to show physicists, in other words, that the label 'test theory' desperately understates the significance and worth of this method.

As far as I am aware, there have been no studies of test theories quite like the study that I undertake in this dissertation. In his excellent physics text *Theory and Experiment in Gravitational Physics* Clifford Will provides theoretical analyses of test theories for relativistic gravitation, and surveys the experimental results used to determine test-theory parameters. In more recent articles Will also discusses test-theory constructions for relativistic kinematics. What Will does not do—being a physicist rather than a philosopher—is discuss the test-theory method in a general way. He does not compare the method with other approaches to confirmation, nor does he analyse the conceptual presuppositions of the method. By not investigating these matters Will fails to provide physicists with a clear sense of just how powerful the method is and how important it could be for the future unification of the fundamental physical interactions. In this dissertation I will address these issues on which Will remains silent.

A small number of contemporary philosophers of science, including Jon Dorling, Clark Glymour, John Norton, John Earman, and William Harper, address in works for philosophers what physicists today call test theories. In characterising the work of physicists who use test theory-type constructions, these writers employ philosophers' terms such as "demonstrative induction", "eliminative induction", and "bootstrapping". In this dissertation, I, by contrast, use the terminology that is favoured by physicists. In so doing, I emphasise the attention physicists themselves draw to their own methods, and I thereby avoid some arguably undesirable connotations of the philosophers' terminology. I also examine in this dissertation inherent limitations of test theory methodology, limitations which I believe have not been adequately discussed, or even sufficiently acknowledged, by other philosophers of science. Thus, in addition to making greater connection to the language of contemporary physics, I seek a more balanced view of what I regard nonetheless as an important method of physical science.

Acknowledgments. Gabrielle Riley has been my companion and provider while I have been working on this dissertation. She has put up with a lot, and has postponed her OE because of me. Without her friendship, support and encouragement my doctoral studies would have been much less enjoyable than they were. I am very grateful to her, and give her my warmest thanks.

With much pleasure I thank my supervisor, Doctor Philip Catton, for his patience, excellent guidance, and for his generous and thought-provoking instruction. Having originally been trained as a physicist, I appreciate his efforts in bringing me up to speed on relevant philosophical issues, and for persevering while I developed my writing skills to an acceptable standard. I also acknowledge the influence his ideas have had on this dissertation, particularly as regards the philosophical importance of Newton and bootstrap methodology.

I give many thanks to Doctor Kumar Vetharaniam, whose active research on test theories for relativity has been a significant source of inspiration for this dissertation. I have also valued his friendship and encouragement, especially during some of the less productive periods of my doctoral studies.

I have enjoyed sharing a room in the department with Jonathan Bywater. He deserves thanks for impressing upon me that there other things of interest one can philosophise about apart from physics. I have appreciated the relief from doctoral studies provided by our discussions on non-philosophical topics of interest to us both. I am also grateful to Stephen Sharp for providing welcome distractions during the final writing up, and for helping with the printing out.

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CHAPTER 1

Introduction

In this dissertation I examine a number of theoretical frameworks which physicists have devised to strengthen and clarify the empirical credentials of fundamental physical theory. Although these frameworks involve a wide range of physical quantities—including forces, actions, matter fields, and spacetime metrics—they all share features which distinguish them as test-theory frameworks. My primary aim in this chapter is to describe the general character and function of test theories, and to indicate why test theories are important to the philosophy of science.

In Section 1.1 I introduce the test-theory idea by describing elements which are common to all test theories, and I indicate how these elements enable test theories to perform the functions they do. In Section 1.2 I discuss the historical origins and general methodological significance of the test-theory idea. In Section 1.3 I present a simple (though unhistorical) example of a test theory to illustrate the test-theory idea and to emphasise features of test-theory methodology which will be important to my philosophical discussion in later chapters of actual historical test theories. Finally, in Section 1.4 I explain and summarise the argumentative structure of my dissertation.

1.1 Test theories in general: character and function

In the present section I give a very general description of test theories, by introducing and defining the following terms which label elements common to all test theories: 'basic equation', 'test-theory parameter', 'conceptual presupposition', 'defining assumption', 'auxiliary assumption'. I also describe the two main uses that physicists today have for test theories. These are (1) to confirm physical theory empirically, in a way significantly different from, and considerably stronger than, the traditional hypothetico-deductive method of confirmation, and (2) to classify rival physical theories according to their empirical content (location in parameter space).

Test theories are mathematical in character and have, as their primary constituent, a *basic equation*. This equation specifies a relationship between some key physical quantity and other physical quantities upon which the first depends. For example, a test theory in Newtonian dynamics would have a basic equation which specifies the relationship between force and distance, say, or between force and velocity. A test theory in relativistic dynamics would have a basic equation which relates a field quantity to other variables. However, a crucial feature of any test theory's basic equation is that it does not specify completely the relationship between its constituent physical quantities. Certain elements of the equation are left arbitrary, and the degree of arbitrariness is quantified by *test-theory parameters*. The reason why these elements are at first left arbitrary is so that they may later be determined empirically in a way I shall describe in a moment.

If each parameter in a test theory is assigned a numerical value, then the basic equation's form is completely specified. In this case, a physical theory is picked out uniquely by the values assigned.[1] We may say that a set of values for n test-theory parameters represents a point in n-dimensional parameter space. Typically, this space is a continuous space because test-theory parameters typically are real-valued variables. Hence, in its undetermined state a test theory can represent an infinite number of more-or-less closely related rival physical theories.

I distinguish the conceptual presuppositions from the defining assumptions, and from the auxiliary assumptions, of a test theory. The *conceptual presuppositions* of a test theory include the general physical and mathematical principles which must obtain if the test theory is to be at all possible. For Newton's test theory the conceptual presuppositions include his Three Laws of Motion and the postulates of Euclidean geometry. For the PPN Formalism they include the postulates of modern differential geometry and also the doctrine that gravity is best described by a metric theory. The conceptual presuppositions of a test theory specify the general character of the key physical quantity appearing in the test theory's basic equation.

The *defining assumptions* of a test theory specify the unparameterised, or fixed, elements of theory appearing in the basic equation. For instance, a defining assumption of Newton's test theory is that the strength of forces acting between celestial bodies is dependent only on distance from the source and not on direction. Hence, the force variable appearing in Newton's test theory is functionally dependent on distance but not on angle. Evidently, a test theory's defining assumptions specify features shared by all the physical systems to which the test theory can be applied. *Auxiliary assumptions* specify further features, features which, however, restrict a test theory's application to a subclass of the total physical systems. For example, one of Newton's auxiliary assumptions restricts application of his test theory to bodies which have nearly circular orbits. Unlike defining assumptions, auxiliary assumptions do not influence the form of the basic equation, but

[1] This is usually true. However, if a test theory's basic equation contains a truncated infinite series, then specific values of the finite number of parameters appearing may pick out more than one physical theory. To obtain uniqueness, one may have to extend one's test theory by including higher order terms. An example of this state of affairs is provided by the PPN Formalism, the central equation (Equation 3.18) of which contains a truncated expression for the spacetime metric field.

are used to derive observable relations from that equation.

The primary use physicists today have for test theories is decisively to select empirically a physical theory from out of a parameterised set of alternatives. To this end the physicist's immediate goal is to determine the arbitrary features of a test theory's basic equation by determining empirically the values of its test-theory parameters. This happens in the following way. From the basic equation, together with auxiliary assumptions appropriate to physical system under study, are derived expressions which contain observable quantities and test-theory parameters. Comparing these expressions with selected observations determines (up to tolerances set by experimental uncertainties) the values of the test-theory parameters appearing.

Now it is generally possible to derive from these expressions the basic equation, again by way of the auxiliary assumptions. Thus it is generally possible to establish a two-way, or biconditional, relationship between a test theory's basic equation and the expressions derivable from it. The significance of biconditionality is that when we use observations to determine the value of a test-theory parameter, those observations may be said to measure the theoretical element associated with this parameter. Because this measurement is potentially many-sided, because it is possible for seemingly unrelated phenomena to return the same value for a test-theory parameter, and thus overdetermine that parameter, test theories allow observations very strongly to confirm elements of physical theory which have been quantified by test-theory parameters.[2] In this dissertation I will refer to numerous examples, both historical and contrived, of such confirmations.

Besides allowing observations to measure theoretical parameters, test theories can also provide an illuminating way of classifying rival physical theories. The "distance" of one theory from another in parameter space can provide a measure of how close they are in terms of their empirical content. How well behaved this measure is depends on the nature of test-theory parameters involved. If one parameter, say, is an exponent, while another is a coefficient, then the measure may not be all that well behaved, because small differences in the value of an exponent can have vastly greater empirical consequences than small differences in the value of a coefficient.[3]

How illuminating our test-theory measure is depends both on the number and nature of parameters involved, and on the conceptual presuppositions of the test theory. Classification of relativistic gravity theories by the ten-parameter PPN Formalism (see Section 3.7) is very illuminating, because the field equations of these theories can appear to the eye quite different in form and yet diverge only slightly in what phenomena they predict.

[2]There are also cases where a single phenomenon affords an especially sensitive measure of some piece of theory, due to the functional form of the expressions in which a test-theory parameter appears. I give an example in Section 2.4.

[3]For example, altering slightly the exponent in Newton's inverse-square law can imply that the apsides of planetary orbits will exhibit large precessions, whereas similarly altering the value of the gravitational constant implies no comparably striking effect on orbital characteristics.

1.2 Origins of the test-theory idea

The name 'test theory' will be unfamiliar to most philosophers, and also to physicists not working on the empirical verification of gravity and spacetime theories.[4] The physicist L. I. Schiff [72] referred in 1967 to the application of a test theory for relativistic gravity as a "quantitative method" and also as a "test method".[5] But the earliest use of the name 'test theory' seems to have been made by the physicists Reza Mansouri and Roman Sexl [52] in their 1977 paper "A Test Theory of Special Relativity".

Mansouri and Sexl do not define 'test theory', but employ the name to refer to a theoretical framework they have devised for analysing tests of relativistic kinematics. The authors acknowledge the pioneering work of H. P. Robertson [68] in the development of such frameworks, and they also refer to "a well developed test theory" for relativistic gravitation.[6] This latter is the so-called Parametrized Post-Newtonian (PPN) Formalism, devised by Ken Nordvedt and Clifford Will and based on the test theory discussed by Schiff.[7] Evidently, Mansouri and Sexl regard their theoretical framework not as an isolated object, but as belonging to a class of objects to which they refer collectively as 'test theories'. Since the publication of Mansouri and Sexl's paper, other physicists have begun referring to test theories.[8]

It is a pity that Mansouri and Sexl do not specify the origin of the label 'test theory', for this name is intriguing. I have already indicated (see Preface) how it understates the value of the test-theory method. Test theories can do more than simply facilitate the empirical testing of already existing physical theories. They can in fact help physicists discover new theories. Also, in an important sense test theories are not theories, because unlike normal scientific theories they are not used to make predictions about the world. Rather, physicists use test theories to measure empirically elements of physical theory. What is more, test theories can represent whole classes of rival physical theories within their structures, and for this reason alone it seems legitimate to regard them not as theories in the usual sense but as theories of theories, though even this term has unfortunate connotations.[9]

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[4] At least one philosopher has used the name in a somewhat different sense from that in which physicists are now using it. For J. D. Trout [78] a "test theory" is any physical theory which is the subject of empirical testing, and is distinguished from what Trout calls "remote theories"—theories apparently unrelated to the test theory, but which may need to be conjoined to it to facilitate testing.

[5] See Section 3.6 for further details.

[6] I examine Robertson's test theory in Section 3.5.

[7] Mansouri and Sexl cite a paper by Will which gives a detailed exposition of the PPN Formalism.

[8] For example, J. Vargas [80], G. H. Abolghasem et al. [1], I. Vetharaniam and G. Stedman [81], C. Will [88].

[9] For example, it suggests that test theories provide a philosophical account of the character and function of physical theories, which they certainly do not do.

Use of the names 'test theory' and 'test method' by practising physicists is significant. Physicists refer routinely to ways of performing mathematical derivations within physical theories as methods of some kind or other. But, unlike philosophers, they rarely dignify with a name the way in which physical theories are confirmed, or disconfirmed, empirically. When physicists begin to employ such names on a regular basis we can be fairly sure that something important is happening. Either a methodological discovery has been made or physicists have come to recognise in the practice of their discipline regularities of sufficient moment to warrant a distinguishing title.

I think the latter case is true of the test-theory method. Test-theory-type constructions existed long before Mansouri and Sexl began using the name 'test theory'. Robertson developed his in 1949, Arthur Eddington posed a rudimentary test theory for relativistic gravitation in 1922, and even Newton, I will contend, had a test theory which he used to argue for the inverse-square law of gravity.[10] It appears that the name 'test theory' entered current usage only once a variety of test-theory-type frameworks were being applied in the actual practice of physics. The advent of especially sophisticated test theories, like the PPN Formalism and Mansouri and Sexl's test theory for relativistic kinematics, also contributed, I believe, to a recognition of the methodological importance of these frameworks.

In this dissertation I will argue, on the basis of recent developments in the philosophy of science, that both the construction of test theories and the importance that physicists evidently attach to them indicate just how sophisticated contemporary physicists can be about methodology. In my view, this result undermines the belief, held by many (though not all) philosophers of science, that scientists are methodologically unreflective. Indeed, I will argue that test-theory methodology indicates just how uninformed some philosophers have been about scientists and about the methods of science.

1.3 A simple test theory

The test theory I present in this section, to illustrate the test-theory idea, is both simple and rather abstract. This test theory applies not to any actual physical system, and the quantities which appear in it are not actual physical quantities. In its simplicity and overall character it perhaps most closely resembles Newton's test theory (which I examine in the next chapter). However, in contrast to Newton I have stripped away all the subtleties of applying my simple test theory to any real physical system. By doing this I aim to give the reader some idea of what a test theory looks like, before discussing the intricacies of applying actual test theories to the real world.

Suppose we are interested in a physical system S, and have good reason to believe that causal or other key physical relations which obtain between the constituents of S will be well described by a physical quantity Q. Other,

[10] For a discussion of Eddington's test theory see Section 3.6. The evidence for Newton's test theory is presented in Section 2.1.

independently variable, quantities r, s, t, which are observable, specify further physical relations between the constituents of S and also, perhaps, properties intrinsic to those constituents. Q satisfies i general physical principles $f_i(Q, \dots) = 0$, where the f_i are partial functions in Q. It also satisfies certain mathematical constraints which specify how a given Q may be manipulated and combined mathematically with r, s, and t, and with other Qs. The physical principles and mathematical constraints which Q satisfies will constitute the conceptual presuppositions of our test theory.

We want to understand the specific character of the causal relations which obtain in S. This means that we need to determine the functional dependency of Q on other physical quantities which describe our system. In fact, we already suspect that there exists such a relationship between Q and r, though we are uncertain of its precise character. According to the hypothetico-deductive method of testing we should, at this juncture, conjecture a definite relation between Q and r (e.g. $Q \propto r$), and then test that relation by using it to explain known behaviour of the system and to predict novel phenomena. However, this is not how things are done in test-theory methodology. In test-theory methodology the idea is not to hypothesise a definite relation between Q and r, but to derive this relation empirically from phenomena.

To do this, we need first to formulate our test theory's basic equation:

$$Q \propto r^{\delta} \,, \tag{1.1}$$

where the real-valued test-theory parameter δ quantifies that element of theory which we wish to determine by the test-theory method. A defining assumption of our test theory is that Q relates to r in the simple way displayed in Equation 1.1, and not in some more complicated fashion (which, say, involves other variables inseparably). The next step is to derive from Equation 1.1 expressions which can be compared with phenomena in S, and which will thereby allow us to determine the parameter δ. If, for example, the second of the general physical principles which Q satisfies is $f_2(Q, s, t) = Q - st^2 = 0$ (and remembering that s and t are independently variable quantities), then

$$\begin{aligned} & Q \propto r^{\delta} \iff s \propto r^{\delta} \\ \text{and} \quad & Q \propto r^{\delta} \iff t \propto r^{\delta/2} \,. \end{aligned} \tag{1.2}$$

Finally, given that the relations between r, s and t are observable relations (since r, s and t are observable quantities), we can, from observing phenomena in S, determine the value of δ. We can thus "measure", and thereby confirm, that aspect of the theoretical relation between Q and r which δ quantifies.

It is moreover evident how, by way of our test theory, disparate-seeming phenomena may overdetermine, and thereby very strongly confirm, the theoretical element which is parameterised by δ. If, for example, it is independently found both that $s \propto 1/r$ and that $t \propto 1/r^2$, then we have very strong reason to believe that $Q \propto 1/r$, i.e. that $\delta = -1$. For this value of δ has been fixed as it were from two sides by disparate phenomena, which is just a huge coincidence unless δ does indeed equal -1.

Besides the possibility of empirically overdetermining elements of physical theory, our simple test theory has further features of which we should take special note. Some of these features distinguish the test theory from more realistic test theories which I examine later, while other features are important to my philosophical discussion of test theories.

Firstly, I have made no use of auxiliary assumptions in my toy application of this test theory. I have derived from its basic equation relations between observable quantities using only the general physical principle f_2. This is in keeping with my present aim to keep things simple. Later, I will explain why Newton needed auxiliary assumptions to apply his test theory to celestial phenomena, and thus clarify why such assumptions are generally needed in test-theory methodology.

Secondly, the test theory in this section, like Newton's test theory, contains just one test-theory parameter, δ. In contrast, modern test theories often contain a number of such parameters which quantify different aspects of some theoretical relation. Nevertheless, a single parameter is sufficient to show how the test-theory method enables physicists to select empirically a theory from among a range of alternatives. For, the theoretical relation between Q and r can be regarded as a physical law, and different values of the parameter δ yield different physical laws, each of which is the basic constituent of a different physical theory.

Thirdly, our simple test theory shows how given phenomena may be relevant to some very specific part of theory. Because δ appears on both sides of Equation 1.2, it is clear that the observable relations between s and r and between t and r in Equation 1.2 are relevant just to the exponent in our test theory's basic equation. In contrast, the hypothetico-deductive method gives us no indication of exactly which part of theory a phenomenon might be relevant to.

Fourthly, because it shows how quite disparate-seeming phenomena can be relevant to the same element of physical theory, our test theory establishes a theoretical connection between those phenomena. It shows how unalike phenomena may be conceived together, or theoretically unified. In calling the subject S of our test theory a system (as I did at the start of this section) I presupposed a connectedness, or unity, among the constituents of S. However, the connectedness that there is in S may only be suspected by physicists and may yet require demonstration. I will show in Chapter 2 how Newton used his test theory to demonstrate for the first time that such a connectedness obtains among different phenomena in what henceforth, with certainty, could be called the Solar System. Thus, I will show how Newton used his test theory not merely as a tool for theoretical confirmation but, more significantly, as a tool for theoretical discovery.

Fifthly, it is clear that our simple test theory for Q would not be possible were it not for the general physical principles (and mathematical results) which tell us what Q is, in general, like. Typically, these principles are not themselves confirmable by the test-theory method, though there are exceptions, as I will later show. In this dissertation I will emphasise that physicists usually settle on the principles needed to make test theories possible in a way which is not straightforwardly empirical, but more closely approximates the conceptual and metaphysical work of philosophers. Thus, I will use "test-theory determinability" as a demarcation criterion to separate the plainly empirical from the more philosophical aspects of practice in physics.

1.4 Précis of following chapters

In this dissertation I first discuss historical (Chapters 2 and 3), then conceptual (Chapter 4), and finally methodological (Chapter 5) issues on which test theories have important bearing. Because I believe that the warrant for test-theory methodology is simply its utility in furthering the aims of science (see Preface) I begin by examining fruitful applications of test theories from the history of physics. I will conclude from my historical studies that the warrant for the test-theory method is indeed very great, thereby demonstrating that the method is both an appropriate one for physicists to continue using, and a worthy subject for philosophical reflection.

In Chapter 2, I contend that Newton used a test theory to argue for his theory of universal gravitation. I describe this test theory, and defend the cogency of Newton's application of it. I also shed light on Newton's test theory by discussing it in relation to Newton's own philosophical reflections on scientific inquiry, i.e. to Newton's exposition of what he called "experimental philosophy". I argue for the importance of Newton's test theory if we are properly to understand and evaluate the priority dispute between Newton and Hooke over the discovery of the inverse-square law. Newton's test theory, I will conclude, may be among the simplest of all historical test theories, but Newton's application of it is the most profound and far-reaching use of any test theory. Newton used his test theory not merely to confirm already widely accepted physical theory, but to discover new theory—theory which itself is of deep physical significance. Newton's fruitful application of the test-theory method constitutes early evidence (probably the earliest in history) that this method is capable of delivering physical theories of great explanatory and unifying power, thereby furthering considerably the aims of science and affording impressive warrant for the method.

In Chapter 3, I argue that Einstein's advance beyond Newtonian physics had similar philosophical motivation, and was achieved by similar means, to Newton's own much greater advance beyond the mechanistic physics of his day. Although Einstein did not, strictly speaking, employ a test theory, he nearly did, and I will contend that much of the inspiration for modern test theories of relativistic kinematics and gravitation derives from Einstein's own very Newtonian approach to empirically justifying physical theory. The test theories I examine in Chapter 3 are generally more complicated than Newton's test theory, in that they contain a greater number of test-theory parameters. They provide further evidence of the power of the test-theory method. Their existence and fruitful application implies that the method is not confined to a Newtonian conceptual context, but is of broader scope. By comparing modern test theories with Newton's I determine the extent to which Newton's ways of working have passed into the reflexive practice of present-day physics. I determine, by way of this comparison, the great similarity, or commensurability, of Newtonian to Einsteinian physics, in both their theoretical and methodological aspects. I show, in particular, how it is possible to represent both Newtonian and Einsteinian kinematics, within a single test-theory framework. Thus, I present a unified view of theory and practice in physics since Newton's time.

Whereas in Chapters 2 and 3 I demonstrate the power and broad scope of the test-theory method, in Chapter 4 I discern the method's deepest limits. I acknowledge that test theories enable us to determine empirically many elements of spacetime structure previously regarded by philosophers as either metaphysical or conventional in character. However, I show how some of the more constitutive elements of spacetime structure at present fall beyond the ken of test-theory methodology, though some of these may be determined once further methodological principles, of a less formal nature, are also brought to bear. I show in addition that test-theory methodology does not help us at all to choose between efficient and final cause conceptions at the level of fundamental physics. Those elements of physical theory which test-theory methodology reaches only with assistance, or does not ever reach, I identify as conceptual presuppositions of test theories. Those conceptual presuppositions which are not open even to partial determination by the test-theory method, are, I shall maintain, the proper preserve of metaphysics and conceptual analysis rather than of the merely empirical part of scientific inquiry.

In Chapter 5, I compare and contrast test-theory methodology with influential accounts of confirmation from the philosophy of science. By means of this comparison I am able better to ascertain the strengths of test-theory confirmation. I am also able to reinforce and clarify further my claims in Chapter 4 about the limits of test-theory methodology. In Chapter 5 I use test-theory methodology to defend some "positivist" doctrines against "post-positivist" criticism. For example, I defend (i) the reductionist belief that specific pieces of evidence are relevant to specific pieces of theory, (ii) the falsificationist belief that crucial falsifying experiments are possible, and (iii) the anti-relativist view that there are theory-independent ways of assessing rival conceptual schemes. I argue that test-theory methodology is closely related to, but still distinct from, certain neo-positivist accounts of confirmation, such as "bootstrap" methodology and eliminative induction. Despite their differences, I acknowledge that bootstrap and test-theory methodology share many salient features, including the important presupposition that nature is systematic, or unified.

In Chapter 6, the final chapter, I use the results of previous chapters to conclude: (1) that physicists can be methodologically reflective, and that philosophers need to acknowledge this; (2) that aspects of practice which physicists themselves have made explicit are important for our understanding of the methods, foundations and history of physics; (3) that these same aspects are relevant to the modern unification programme in theoretical physics. Regarding (1), I conclude from my historical case studies, and from my comparison of test-theory methodology with philosophical accounts of confirmation, that philosophers of science need to recognise, on their own terms, just how sophisticated physicists can be when it comes to the empirical justification of physical theories.

Regarding (2), I conclude that test-theory methodology indicates that certain older philosophical accounts of empirical confirmation and meaning, which are widely rejected today, have salient and illuminating features not shared by some more modern accounts. I conclude that test-theory methodology shows how certain fundamental elements of physical theory, previously thought to be metaphysical or conventional in character, are in fact empirically conditionable. I conclude also that the classical and modern phases of inquiry in physics, usually considered disparate, are in fact unified at the level of test-theory methodology.

Regarding (3), I conclude from my historical case studies that test theories have already met with striking success in delivering theories of great unifying power. On this basis I propose that the test-theory method is an appropriate method to unify the four fundamental physical interactions. Despite their methodological acumen—which I do not doubt, and indeed strive to acknowledge—contemporary practitioners of the test-theory method (in contrast to Newton) have not, I claim, discerned the method's potential for further theoretical discovery and unification. Drawing on my results from Chapter 4, I emphasise the necessity of a suitable conceptual framework for test-theory unification of the fundamental interactions to be possible, and I suggest that the "philosophical" search for such a framework is of greater urgency than further empirical investigation of high-energy phenomena. I suggest, in other words, that theoretical physics today needs better philosophy more than it needs bigger particle accelerators.

CHAPTER 2

Newton's test theory and his experimental philosophy

The principal aim of this chapter is to show that Newton devised, and convincingly used, a test theory to establish the inverse-square law of gravity. Recently, philosophers of science have taken much interest in Newton's argument for the law of gravity. They have commented on the strength of his argument and on its systematic features.[1] They have pointed out how it involves explicitly Newton's professed method of deduction from phenomena, and also how poorly it fits traditional philosophical accounts of scientific reasoning.[2] In addition, they have shown that Newton's argument is not an isolated and unusual form of reasoning, and that arguments from elsewhere in physics, and even from other sciences, conform remarkably well to Newton's example.[3]

Nevertheless, my view is that these philosophers have not gone far enough, at least with regard to physics, in establishing that Newton's method has importantly shaped the contemporary practice of science. By showing, in this chapter, that Newton's argument for the inverse-square law of gravity involves crucial and cogent use of what present day physicists call a test theory, I establish a deep commonality between Newton's methods and the methods of modern physicists. I also examine in this chapter the conceptual and philosophical presuppositions of Newton's test theory, with a view to later comparing the motivation for this test theory with that for more recent test theories.

In Section 2.1 I confirm that Newton did in fact possess a test theory, and, but for one small point of unclarity, that he seems consciously to have possessed one. In Section 2.2 I examine the defining and auxiliary assumptions of Newton's test theory, necessary for its existence and application. In Section 2.3 I discuss Newton's "Phenomena", which he employed to measure the value of his test theory's sole parameter. In Section 2.4 I look at Newton's application of his test theory, the results he obtains, and whether he is in fact justified in obtaining these results. In Section 2.5 I consider the

[1] Glymour [31]; Harper [38].
[2] Glymour [31]; Catton [12].
[3] Glymour [31]; Dorling [15]; Catton [12].

place of Newton's test theory in his overall argument for universal gravitation, emphasising its role in making later stages of the argument possible. In Section 2.6 I discuss the conception of force on which Newton's test theory rests, with a view to determining how one should interpret the results of this and other test theories. In Section 2.7 I argue that Newton's test theory arose from his own peculiar understanding of the aims and methods of science, that is, from what Newton called "experimental philosophy". In Section 2.8, I use Newton's test theory to show how the priority dispute with Hooke over the discovery of the inverse-square law can best be understood in terms of Newton's advance beyond the orthodox epistemological doctrines of his contemporaries.

2.1 Newton used a test theory?

Newton's argument for the inverse-square law of gravity is contained in Propositions I–VII, Book III of *Principia* [57]. A test theory does not appear explicitly in this argument, but Newton does invoke propositions from Book I in a way which, in my view, introduces a test-theory-type construction.

According to my general description of test theories in Section 1.1 every test theory possesses a basic equation containing undetermined parameters. The basic equation of Newton's test theory appears first in Corollary VII, Proposition IV, Book I of *Principia*. It appears later, and in slightly different notation, in Cor. I, Prop. XLV of the same book. This equation states that the centripetal force F_c acting on a body varies in some, as yet undetermined, way as the distance r of the body from the centre of force. That is,

$$F_c \propto \frac{1}{r^{2n-1}} \,, \tag{2.1}$$

where n is a real-valued parameter. The relatively complex form $2n-1$ for the exponent is appropriate, because certain important expressions derivable from Equation 2.1 then take on relatively simple forms. One of these expressions, which plays a crucial role in Newton's argument for the inverse-square law, is in Cor. VII, Prop. IV explicitly related to Equation 2.1. This expression states a simple relationship between the orbital period T and radius R of a body travelling along a circular path under the influence of a centripetal force. Specifically, in Cor. VII, Newton states that

$$T \propto R^n \iff F_c \propto \frac{1}{R^{2n-1}} \,. \tag{2.2}$$

Using Newton's Second Law of Motion one can derive mathematically each of the two expressions in Equation 2.2 from the other. The parameter n appears in both expressions, so that if one knows its value in one of these expressions, then the form of the other expression is determined. For example, if one knows that for some body in circular motion $T \propto R^{3/2}$, then $F_c \propto 1/R^2$, and conversely.

Another expression derivable from (and from which one may also derive) Equation 2.1 can be found in Cor. I, Prop. XLV, Book I. This corollary establishes a relationship between the parameter n and the orbital precession of bodies in elliptical-type orbits "approaching very near to circles". Specifically, in Cor. I, Prop. XLV, Newton states that

$$n = -\tfrac{1}{2}\left(\frac{\theta + 360}{360}\right)^2 + 2 \iff F_c \propto \frac{1}{A^{2n-1}} \ . \qquad (2.3)$$

Here, θ is the angle of precession (in degrees)—that is, the angle, subtended at the more distant focus, between any two successive apsides. The quantity A is the *altitude* of the orbiting body, which is its distance from the focus of attraction at any time.[4] If the precession is forward, then θ is positive, if backward, then negative. Quiescent apsides (where $\theta = 0^\circ$ or $\theta = 360^\circ$) will obtain for both an inverse-square and a directly proportional law of force.

In my view it is natural to interpret Equations 2.2 and 2.3 as belonging to a single test-theory construction. The expressions for centripetal force in these equations are the same, in that they both contain the undetermined parameter n, which, by taking on different values, determines different force laws, each of which is the fundamental equation of a different physical theory. This feature—of several physical theories being represented parametrically within a single formal structure—is a distinguishing feature of test theories. Another distinguishing feature is that the determination of the theoretical parameters appearing be an empirical determination. In Section 2.4 I will show that in his argument for the inverse-square force law of gravity Newton determines empirically the parameter n appearing in Equations 2.2 and 2.3.

Thus, in my view, Newton not only devised a test-theory-type construction, he also used it in his argument for the law of gravity. These facts are, nevertheless, not immediately evident in *Principia*, and for the following reasons: (1) a test theory does not appear explicitly in Newton's argument for the law of gravity; (2) Newton never cites the corollary to Prop. IV, Book I which contains his test theory's basic equation, but only the preceding corollary, which contains the particular determination ($n = 3/2$) of Equation 2.2 relevant to his argument for the law of gravity; (3) the wide separation (and slightly different notation) of Equations 2.2 and 2.3 in Book I is not conducive to our conceiving them as parts of a single test-theory construction. Indeed, Newton himself, it seems, did not conceive these expressions as part of a single construction.

It is important to realise that (3)—which points to some unclarity in Newton's mind about his test-theory construction—is, nevertheless, not a criticism of Newton. I stated in Section 1.2 that the test-theory idea emerged in modern times only once the use of test-theory frameworks had become

[4] I have not retained completely Newton's original notation in Equation 2.3, and I have performed elementary manipulations to obtain a form for this equation which more closely approaches the form of Equation 2.2.

a regular part of physical practice. Newton was probably the first scientist ever to use such a framework. He was certainly the first to obtain deep and far-reaching results by using one. It would be demanding too much, I believe, to demand of Newton that he make completely transparent for all to see the nature of his innovation. Nevertheless, one would seriously underestimate Newton's stature as a methodological thinker (indeed as a philosopher) were one to believe that Newton somehow unreflectively used what I have called his test theory. In Section 2.7 I show that Newton understood very well those aspects of his reasoning which involved his test theory implicitly.

2.2 Defining and auxiliary assumptions

Newton could not have stated the basic equation of his test theory, Equation 2.1, without having in mind appropriate defining assumptions. He could not have applied this equation without specifying appropriate auxiliary assumptions. Evidently, the defining assumptions of Newton's test theory are (1) that the force acting is centripetal (i.e. towards a point), and (2) that the force's strength varies only with distance (and not with direction) from the origin of force, though this includes the degenerate case where the strength is constant with distance (i.e. where $n = \frac{1}{2}$). In Section 2.4 I discuss Newton's justification for these defining assumptions in applying his test theory to the Solar System.

Auxiliary assumptions specify features of some particular physical system. In effect, they convert Newton's general test theory, represented by Equation 2.1, into a *model* of that test theory, just as the conjoining of initial and boundary conditions to a set of physical laws is often regarded as specifying a physical model of those laws. In effect, the test theory's auxiliary assumptions endow it with meaning, making it relevant to specific physical systems. At one level higher, the test theory itself may be regarded as a model of Newton's general dynamical principles, albeit with certain undetermined features.

Auxliliary assumptions enable one to derive from Equation 2.1 certain important expressions relevant to that system. They also allow one to derive, conversely, Equation 2.1 from these expressions. Examples of these expressions can be found in Equations 2.2 and 2.3. They state relationships between "observable" quantities of the system, that is, between quantities like T, R, and θ. (Precisely what 'observable' means in this context I will clarify in Section 2.3.)

The physical system pertaining to Equation 2.2 involves a body held in a circular orbit by a single centripetal force. Hence, in this case the auxiliary assumptions of Newton's test theory will specify the singularity and centripetality of the force acting and the shape of the body's trajectory. The same will be true for the system pertaining to Equation 2.3, which however involves not a circular but an elliptically shaped orbit.

There is a further auxiliary assumption, in addition to those of the kind just mentioned, which is required in order to apply Equation 2.1 to real physical systems. This assumption concerns choice of reference frame. Newton makes the same choice—a frame attached to the centre of centripetal force and rotationally fixed relative to the distant stars—for all applications of his test theory, this frame being eminently suitable to the kind of real systems he analyses in *Principia.* Because Newton's derivations typically are geometrical, rather than algebraic, in character, Newton is not required to specify a coordinate system adapted to this frame.

Newton's test theory contrasts with the toy test theory I presented in Section 1.3 in that it requires auxiliary assumptions if it is to be applied successfully to the Solar System. The reason is that relations derived solely from Equation 2.1 and Newton's dynamical principles are not directly observable relations for this system. For example, it follows immediately from Equation 2.1 and Newton's Second Law of Motion that $a \propto 1/r^{2n-1}$, where a is the acceleration. However, it is not possible directly to measure the acceleration of a planet or moon. Nevertheless, we know that for circular orbits $a \propto R/T^2$—where R and T can be measured—and from this fact it is straightforward to obtain Equation 2.2. It is because we require relations between directly observable quantities, that we must, in general, use auxiliary assumptions when applying test theories to real physical systems.

Now it is true that the auxiliary assumptions which Newton, in effect, uses with his test theory are simple and in fact specify rather ideal physical systems. However, this fact is not a cause for worry in Book I of *Principia*, for Newton's propositions in that book deal primarily with motion in ideal physical systems. Problems arise, or least appear to arise, when in Book III Newton applies his test theory, with these same simple assumptions, to real physical systems, namely to the Solar System and its constituent subsystems. I will discuss these problems in Section 2.4.

2.3 Newton's Phenomena

Newton employs his test theory in Book III of *Principia*, to argue for the inverse-square character of forces acting in the Solar System. Evidently, Newton's various arguments are not pure a priori arguments. They have in fact a large empirical component. Newton himself maintained that his three Laws of Motion are ultimately conditioned by experience, implying that his test theory, which is a high-level specification (or model) of those laws, also has empirical content.[5] However, the most conspicuously empirical contribution to Newton's arguments is provided by what he called Phenomena, statements of which immediately precede Prop I of Book III.

Newton's Phenomena are mathematically expressed relationships obtaining between physical quantities which specify the character of various planetary and lunar orbits. An example is Phen. Ib, which states that the

[5] The passage in which Newton affirms the empirical status of his Laws of Motion is quoted by H. S. Thayer [76] (p. 6). For a partial quote, see my Section 2.7.

orbital periods T of Jupiter's (four) moons are all proportional to the 3/2th power of their respective distances from Jupiter's centre, that is, $T \propto R^{3/2}$.[6] Another example is Phen. V, which states that the areas A swept out by the (five) planets, by radii drawn to the Sun, are proportional to their respective times of sweeping, that is, $\ddot{A} = 0$ (Kepler's Second Law).

These Phenomena, which Newton supports by appealing to observations made by himself and by various eminent astronomers, (including Kepler, Boulliau and Cassini) have two salient characteristics: (1) they are regarded by Newton as robust, and (2) while they are obviously conditioned by observations, they are nonetheless too theoretical in character to be dubbed observations themselves, still less, data. In fact, the extent to which Newton's Phenomena are directly empirically ascertainable varies. For example, Phen. Ib is highly determinable in this way, whereas Phen. V is much less so.

Evidence that Newton regarded his Phenomena as sufficiently robust to use as the basis of his argument for the law of gravity is provided by the very positive endorsements he attaches to them: "this we know from astronomical observations" (Phen. Ia); "all astronomers agree that" (Phen. Ib); "is now received by all astronomers" (Phen. IV); "all astronomers are agreed" (Phen. IV); "a noted proposition among astronomers" (Phen. V).

In striking contrast to these positive endorsements is Newton's remark that "... Kepler knew ye Orb to be not circular but oval & guest it to be Elliptical ..."[7] Newton does not admit into his Phenomena Kepler's First Law, even though he could have used it, in conjunction with Prop. XI of Book I, to mount a further and very striking argument for the inverse-square proportion. The historian of science Curtis Wilson [89] contends that Newton did not regard the ellipticity of planetary orbits as sufficiently well-established to be used in his argument for the law of gravity. Consonant with his stringent experimental philosophy, which I shall examine in Section 2.7, Newton admits, as a basis for his argument, only those phenomena which he regards as robust. That is, Newton admits only phenomena, the truth of which is either plainly evident from his own careful observations or widely received by the astronomical community (or both).

Newton's Phenomena are to be distinguished from mere observations or data. For, they describe regularities that have been found to hold good of a vast number of observations. As such, they are interpretations of observations, and contingent to a greater or lesser extent upon various tacit assumptions of a less observational, more theoretical, nature. For example, the truth of Kepler's Second Law is contingent on, at the very least, Euclidean geometry being an accurate description of the geometry of physical space. Even individual quantities, such as T and R, which make up Newton's

[6] Some of Newton's Propositions, including his statements of Phenomena, have two parts. The first parts I label with an 'a', the second with a 'b'. For example, the second part of Phen. I becomes Phen. Ib.

[7] See Wilson [89], p. 90, for a fuller quotation.

Phenomena, have this latent theoretical character. Clearly, these quantities are not sensible quantities. When, however, I refer to these quantities as observable or as directly observable, as I did in Section 2.2, I mean simply that they are inferable from observations in a rather direct way—that is, by using perhaps some, but not a large number, of high-level background beliefs.

Data, in contrast to phenomena, and even in contrast to observable quantities like T and R, can be fickle, and dependent on the vagaries of experimental apparatus and observing conditions. Data do not have the robustness of phenomena, which derive from analysing and interpreting a large and wide-ranging number of observations. Nor, as Bogen and Woodward [6] have pointed out, do data have the requisite theoretical form to act as explananda in a deductive-nomological argument. The basis of Newton's test-theory determination of the inverse-square proportion needs to be not data but something much more worked-up and robust. That this is so I will at any rate attempt to make clear in the detailed discussion below of Newton's work.

2.4 Newton's application of his test theory

In Props I, II and III, Book III of *Principia* Newton uses his test theory to argue that the forces acting on the satellites of Jupiter and Saturn, on the planets, and on the Earth's moon are all of an inverse-square character. Before applying his test theory Newton needs to justify its two defining assumptions—that the forces acting are centripetal, and have a strength which depends only on the distance (and not the direction) from the force's origin.

Newton establishes the centripetality of the forces acting on the moons of Jupiter and Saturn (Prop. Ia), on the planets (Prop. Ib), and on the Earth's moon (Prop. Ic) from the fact that these bodies all obey the Keplerian law $\ddot{A} = 0$ (Phena Ia, V and VI) and from the geometrical result that a body which satisfies this law about a point is urged by a force directed to that point (Prop. II, Book I). Specifically, Newton determines in each case that the force on the secondary bodies is directed towards the centre of their respective primaries. For example, the force acting on Jupiter is directed toward the Sun's centre.

Surprisingly, Newton nowhere explicitly justifies the second defining assumption of his test theory: that the strengths of the forces in question vary only as the distance from the centres of force. Admittedly, there are but two possibilities that are alternative to this one: the strengths may vary only as the direction of the force, or as both the distance and the direction. Presumably, Newton ruled out these two alternatives on the basis that the centripetal forces acting in the Solar System were found by him to be directed towards spherically symmetric bodies. Such bodies do not invite one to consider direction dependencies.

Having established the centripetality of celestial forces, Newton then applies his test theory to determine the precise character of the distance-dependence of these forces. I will consider each of Newton's three applications in turn.

Application 1: In Prop. Ib of Book III Newton argues from the fact that the orbits of Jupiter's and Saturn's moons obey $T \propto R^{3/2}$ (Phen. Ib and Phen. IIb), and (effectively) from Equation 2.2, to the conclusion that the strength of the centripetal force acting on these moons varies inverse squarely as the distance from the centres of Jupiter and Saturn. Newton's argument is sound, for Jupiter at least, since the auxiliary assumption of his test theory relevant to Equation 2.2—the circularity of orbits—seems to conform accurately to the actual orbits of those moons. According to Newton, the moons' orbits "differ but insensibly from circles concentric to Jupiter".

Application 2: In Prop. IIb, Newton argues from the fact that the orbits of the planets obey Kepler's Third Law, that is, $T \propto R^{3/2}$ (Phen. IV), and (effectively) from Equation 2.2, to the conclusion that the strength of the centripetal force acting on these planets varies inverse squarely as the distance from the Sun's centre. Newton then argues to the same conclusion from the quiescence of the planetary aphelia, and from Equation 2.3. Newton's first argument for Prop. IIb looks dubious, because the auxiliary assumption of circularity pertaining to Equation 2.2 is not satisfied by the actual planetary orbits, which Newton knew to be only approximately circular.

Now Kepler's Third Law is in fact a relationship between the orbital periods T and the *mean* distances r_{MEAN} of planets from the Sun. Thus, in order to apply his test theory to Kepler's Third Law, Newton has had to treat the actual orbits of the planets as circles of radii $R = r_{\text{MEAN}}$. As if aware of the uncertainty of this practice, Newton supports his first argument in Prop. IIb with a further, more powerful argument:

> But this part of the Proposition [i.e., IIb] is, with great accuracy, demonstrable from the quiescence of aphelion points; for a very small aberration from the proportion according to the inverse square of the distances would (Cor. I, Prop. XLV, Book I) produce a motion of the apsides sensible enough in every single revolution, and in many of them enormously great.[8]

This second argument of Newton's is less dubious than the first, because the auxiliary assumption of Newton's test theory pertaining to Equation 2.3 concerns orbits which are elliptical orbits "approaching very near to circles". This assumption corresponds more closely than the assumption of circularity to the actual orbits of the planets. What is more, Newton's second argument

[8] According to Equation 2.3, both inverse-square and directly proportional forces are compatible with quiescent apsides. Newton does not explicitly rule out here the directly proportional force. No doubt he had in mind, however, very general facts about the universe which suffice to rule out such a force.

for Prop. IIb shows how, by the test-theory method, it is possible for a single phenomenon very sensitively to measure an element of theory. In this case, the sensitivity is due to the form of Equation 2.3, according to which $2n - 1 \sim 2(1 - \theta/360)$.

Several questions remain, however. (1) Why did Newton not list the quiescence of planetary aphelia among his Phenomena in Book III? (2) Does not the auxiliary assumption of ellipticality in the second argument contradict Curtis Wilson's view that Newton thought Kepler's First Law too uncertain to use as the basis of an argument for the inverse-square relation? (3) If Newton's second argument for Prop. IIb is really more cogent than his first, why did he even bother including the first argument?

With regard to the quiescence of planetary aphelia, Newton writes as if this phenomenon were well-established. So, it is a genuine puzzle why he did not include it among his Phenomena, though it is a puzzle which does not, I think, undermine the cogency of Newton's second argument for Prop. IIb. In answer to (2) it is well to recognise that for the case of very near circularity, elliptical and oval orbits would differ negligibly from one another in so far as their asides are concerned. Therefore, it is possible, I think, to agree with Wilson's view and still regard as cogent Newton's argument from the quiescence of planetary aphelia.

One answer to (3) might be that Newton wanted to maintain parity with his justification of Prop. Ib, which argued similarly from a period-radius relation for orbits to an inverse-square relation for force. A better answer to (3) would be that Newton regarded the agreement in the conclusions of his two arguments for Prop. IIb as justifying the approximation made in premises of his first argument. The agreement of his two arguments, despite apparently conflicting assumptions, would have afforded Newton great confidence that the inverse-square relation held good for the force acting on the planets.

Application 3: In Prop. IIIb Newton argues from "the very slow motion of the moon's apogee" ($3^{\circ}3'$ per rev.), and (effectively) from Equation 2.3, to the conclusion that the strength of the centripetal force acting on the Moon is inverse-squarely as the distance from the Earth's centre. Like the quiescent aphelia of the planets, the slow motion of the Moon's apogee does not make it onto Newton's list of Phenomena. A more serious problem, however, is that this argument, as it stands, is invalid. For, Equation 2.3 implies an inverse-square force only for a quiescent apogee (i.e., $\theta = 0^{\circ}$).

Newton knew from Cor. I, Prop. XLV of Book I that only quiescent apsides would yield a strict inverse-square relation, unless (as in Cor. II, Prop. XLV) an external perturbing force of appropriate magnitude were also acting. By Cor. I, the apsidal precession of the Moon's orbit added a small factor of $4/243$ (less than 1%) to the exponent in the force law. Newton neglects this factor, however, claiming that it "is due to the action of the sun (as we shall afterwards show) ..."

The historian of science Howard Stein thinks Newton's reason for neglecting the small deviation from the inverse-square proportion

> ... makes the argument leading from the Phaenomena through Proposition IV to Proposition VII itself *formally and explicitly dependent* upon the consequences to be drawn from Proposition VII for its own proper completion.[9]

Stein thinks that the full law of universal gravitation is required to make cogent Newton's argument for Prop. IIIb. Stein's intention is not to show that this argument is viciously circular, but to support the view that Newton himself regarded his overall argument for the law of gravity as rather more hypothetical in character than some commentators today are inclined to admit. Nevertheless, Stein's interpretation the cogency of Newton's application of his test theory to the Moon's motion.

I reject Stein's interpretation of Newton here. It is certainly true that Newton could not, at this early stage in Book III, have reasoned cogently that the Sun specifically was responsible for the deviation from the inverse-square proportion (because there were other candidates for a perturbing body). However, I believe that Newton at this stage could have shown, and indeed did show, that some external perturbing body (or bodies), and not the Earth itself, was responsible for this deviation.

Towards the end of his explication of Prop. III Newton states that the precise inverse-square force on the Moon due to the Earth "will yet more fully appear from comparing this force with the force of gravity, as is done in the next Proposition." Newton's own claim, then, seems to be that his neglect of the small factor in the exponent is justified by results connected with Prop. IV, not, as Stein claims, by consequences drawn from Prop. VII.

In his explication of Prop. IV Newton compares the acceleration suffered by terrestrial pendulums with the acceleration the Moon would suffer at the Earth's surface, were it deprived of all tangential motion and allowed to descend to that region. By Newton's calculations the accelerations turn out equal, *if* it is supposed that the force on the Moon, in descending to the Earth's surface, continually increases inversely as the square of the height. If a different increase in the force were assumed, say, an inverse-square increase augmented by a small term, then equivalence would not obtain. If equivalence had not obtained, that would argue (by Rules 1 and 2 of Newton's Rules of Reasoning) for gravity and the centripetal force on the Moon at the Earth's surface being distinct. If these forces were distinct, however, then bodies should fall to Earth with more than double their observed velocity.

They should so fall, that is, unless the force affecting the Moon acts only in regions other than those close to Earth. However, one can take this possibility seriously only if one also believes, as Newton evidently did not, that the entire force on the Moon arises mechanically from the vortical motion in outer space of some kind of aethereal matter. Newton in fact rejected aethereal accounts of celestial motion on both deep philosophical and straightforward empirical grounds (see Section 2.7).[10]

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[9] Stein [75], p. 220.

[10] Thus, I think Stein is mistaken when he argues that Newton's application, in a way

Hence, in my view, Newton had strong empirical reason to think that the Moon's apsidal precession (and just that apsidal precession, not the whole motion) was due to some body (or bodies) external to the Earth, rather than to the Earth itself. Newton's reason derived not from consequences of Prop. VII, as Stein maintains, but from results much earlier in Book III. Specifically, Newton's reason derived from his "Moon test" contained in the explication of Prop. IV. It is on this basis that I accept as cogent Newton's application of his test theory to the Moon's motion.

All in all, Newton's application of his test theory to argue for the inverse-square character of various celestial forces is an impressive achievement, despite difficulties arising from apparent, though ultimately illusory, conflict between Newton's auxiliary assumptions and the actual character of celestial motions. In the next section I will demonstrate how the impressive results Newton obtained using his test theory were crucial to his argument for universal gravitation.

2.5 The argument for universal gravitation

Thus far I have identified a test theory in Newton's methodology, and I have defended Newton's application of it to specific phenomena in the Solar System. In the present section I examine the broader methodological context which Newton, in *Principia*, gives to his test theory. Specifically, I show how the early deductive steps of Newton's reasoning, in which his test theory importantly figures, make possible later steps which extend his test-theory results, and which are crucial to his argument for universal gravitation. In doing this, I relate Newton's test theory, which is implicit in his practice, to some aspects of that practice which Newton himself made explicit, and in particular to his Rules of Reasoning, which are stated at the beginning of Book III of *Principia*. Later, in Section 2.7, I discuss more fully Newton's test theory from the perspective of his general methodological approach to scientific inquiry (experimental philosophy).

The early steps in Newton's argument for universal gravitation are plainly deductive in character. By means of his test theory, Newton deduces from phenomena that various centripetal forces acting in the Solar System are inverse-square forces. Newton's ultimate intention, however, is to deepen and generalise his test-theory results. His ultimate intention is to show that all of these inverse-square forces are gravitational forces, and to show, moreover, that all bodies whatsoever interact gravitationally by means of such forces.

Newton's first step towards this goal is to demonstrate that the Earth's force acting on the Moon is a gravitational force (Prop. IV), a step which is clearly motivated by Newton's prior application in Prop. III of his test

inconsistent with the existence of a substantial aether, of his Third Law of Motion to celestial bodies, was for Newton a boldly hypothetical step. See Stein [75], p. 217ff.

theory to the Moon's motion. I explained in Section 2.4 how Newton's test-theory argument in Prop. III depends for its cogency on results drawn from the explication of Prop. IV. However, it is also true that the argument for Prop. IV itself would have been without motivation but for prior application of Newton's test theory. For, in the explication of Prop. III Newton determines with his test theory that the total force acting on the Moon is of a strength very nearly as the inverse square of the Earth-Moon distance. It is this result alone which invites him to consider what the acceleration of the Moon would be at the Earth's surface were it to descend to Earth under the influence of an exact inverse-square force. Thus, Newton's argument for extending the reach of terrestrial gravity to the Moon relies for its very motivation on the result of his first applying his test theory to the Moon's motion.

Newton's next step, having shown that the force on the Moon is a gravitational force, is to argue for the gravitational character of other forces acting in the Solar System (Prop. V). To this end he employs Rule 2 of his Rules of Reasoning, which states that "to the same natural effects we must, as far as possible, assign the same causes". Specifically, Newton argues that the motions of celestial bodies

> ... are appearances of the same sort with the revolution of the moon about the earth; and therefore, by Rule 2, must be owing to the same sort of causes; especially since it has been demonstrated, that the forces upon which those revolutions depend tend to the centres of Jupiter, of Saturn, and of the sun; and that those forces, in receding from Jupiter, from Saturn, and from the sun, decrease in the same proportion, and according to the same law , as the force of gravity does in receding from the earth.

According to Newton, the attributes of centripetality and the inverse-square proportion, which celestial forces have in common with the force on the Moon, argue especially strongly for the gravitational nature of celestial forces. The inverse-square proportion, in particular, was determined by Newton's application of his test theory, as I showed in Section 2.4. Thus, Newton's test theory plays a crucial role in determining the property of "sameness" which appears explicitly in Rule 2. The test theory helps Newton establish the conditions necessary for application of the rule. In turn, Newton uses Rule 2 to deepen our understanding of his test-theory results.

Newton employs Prop. V to establish deductively further facts about celestial bodies and about gravity. Using his Third Law of Motion Newton deduces from Prop. V that Jupiter, Saturn, the Earth, and the Sun all gravitate towards their respective satellites (Cor. I, Prop. V), and that all planets gravitate towards one another (Cor. III). In Prop. VI Newton uses his Second Law of Motion to argue, again deductively, from the constancy of gravitational acceleration (observed by Newton both in terrestrial pendulum experiments and in the Jovian System) that gravitational force is directly proportional to the mass of the body upon which it acts.

Finally, in Prop. VII Newton invokes once more his Third Law of motion, this time to deduce that the gravitational force due to the planets is directly proportional to the mass of the body acting. From this result Newton reasons that "[t]here is a power of gravity pertaining to all bodies, proportional to the several quantities of matter which they contain". This last step in Newton's argument is clearly an inductive step. Although Newton does not in Prop. VII explicitly invoke Rule 3 of his Rules of Reasoning—which is his rule of induction,—it is evident from his earlier discussion of Rule 3, near the beginning of Book III, that it is by means of this rule that he establishes universal gravitation.

According to Rule 3, "[t]he quantities of bodies which admit neither intensification nor remission of degrees, and which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever". Newton has found, with the aid of his test theory, that bodies in the Solar System generate an inverse-square gravitational force proportional to their mass. Since all bodies have mass, Newton concludes (by Rule 3) that all bodies generate an inverse-square gravitational force. As with Rule 2, Newton's test theory here establishes conditions which make the application of Rule 3 possible. In turn, Newton uses Rule 3 to extend his test-theory results to all bodies.

Generally speaking, the deductive steps of Newton's argument, involving Newton's test theory, make possible the inductive steps, involving Newton's Rules of Reasoning. While Newton's test theory enabled him to establish the form of the inverse-square law, Newton's Rules enabled him to establish its scope. It is evident from the structure of Newton's argument that a determination of form had to precede a determination of scope. If Newton had not established, deductively, the centripetality and inverse-square character of celestial forces then there would have been little motivation for him to argue, inductively, that all these forces are gravitational forces, and moreover that all bodies interact by means of such forces. In Section 2.7 I will show that Newton understood well both the overall structure of his argument for universal gravitation and the character of its individual parts.

2.6 Newton's test theory and his concept of force

The basic equation, and the defining and auxiliary assumptions, of Newton's test theory I examined in Sections 2.1 and 2.2. I have not yet discussed, however, the conceptual presuppositions of Newton's test theory. In particular, I have not discussed Newton's Laws of Motion. Without these laws Newton could not have employed his test theory, nor even coherently stated it. While the Laws of Motion are necessary for the very possibility of Newton's test theory, it is my view that this test theory, in turn, illuminates the character of his Laws. In particular, it illuminates the character of their central concept, the concept of 'force'. In this section I will discuss the relevance of Newton's test theory to the question of how we should properly interpret this fundamental physical concept, in light of the conflicting views expressed by Newton and his contemporaries on the issue. I will conclude that while there is no reason to believe that Newtonian forces are substantial entities, nevertheless Newton's test theory shows why we should believe that these forces represent real physical relations between bodies.

What is already clear from Newton's own definitions is that his dynamical conceptions of both 'mass' and 'force' are abstract in character. According to Definition I, Book I of *Principia* "[t]he quantity of matter [or mass] is the measure of the same, arising from its density and bulk conjointly". According to Def. V "[a] centripetal force is that by which bodies are drawn or impelled, or any way tend, towards a point as to a centre", and in Defs VI, VII, and VIII Newton provides three complimentary definitions for the quantity of centripetal force. Evidently, 'mass' and 'force', as they appear in Newton's Laws, are mathematical quantities. As such, they describe neither the essence of matter nor the essence of physical relations obtaining between material things. They do not pretend to specify, for example, how the existence of material forces and gross material objects might arise mechanically from the motion of microscopic particles.

Both Newton and his contemporaries on continental Europe acknowledged the abstractness of his dynamical conceptions, yet they drew from this fact very different conclusions. While Huygens and Leibniz admired very much the "beautiful discoveries" contained in Newton's *Principia*, they rejected the book's central concept. Huygens found the notion of centripetal attraction "absurd", while Leibniz was dismayed that Newton had not proceeded to demonstrate the mechanical cause of gravity, namely, the aethereal vortex.[11]

To mechanical philosophers like Huygens and Leibniz Newton had not, in *Principia*, advanced the cause of mechanical philosophy, which was the explanation of natural phenomena by reference solely to microscopic particles in motion. From their perspective, Newton's dynamical conceptions were plainly too abstract to assist in attaining this goal. For example, Newton had not with his conceptions explained how "attractions" between bodies could arise from the motion of aethereal particles, and Huygens, for one, was unable to see how such an explanation was even possible. Indeed, Huygens and many others suspected that in advancing a concept as seemingly unintelligible as 'centripetal force' Newton had betrayed mechanical philosophy, and returned to the animism of the ancients, and, in particular, to the so-called "occult" quantities of Aristotle and others.

Aristotle's mode of explaining the behaviour of bodies was to attribute to them an inherent desire or tendency to obtain their rightful state in the Universe. For example, the downward motion of certain imperfect bodies, designated "heavy", was explained by their desire to reach their proper place at the centre of the Earth, the place farthest removed, in Aristotle's cosmology, from the heavenly dwelling of God. Mechanical philosophers, notably

[11] For evidence of Huygen's and Leibniz's reaction to *Principia*, see Westfall [84], p. 472.

Descartes, regarded Aristotle's attempts to explain natural phenomena by appeal to such occult (i.e. hidden) propensities in bodies as anthropomorphic, mysterious, and, consequently, non-explanatory. According to mechanical philosophers such attribution of desire does not explain motion so much as it explains it away.

In contrast to Aristotle and the ancients, mechanical philosophers demanded that explanations of natural phenomena be intelligible to reason, that they not refer to mysterious powers hidden within bodies. Mechanical philosophers explained, or at least attempted to explain, bodily motion in terms of the action by contact with the body of external agents. In particular, the motions of the planets were to be explained by reference to the constituent particles of a single space-filling fluid, the aether, which circulated about the Sun and impinged upon the planets, setting them into motion. Just as a leaf is carried downstream by the action of water particles impinging upon it, so the planets were carried round the Sun by the aethereal vortex. No mysterious powers or anthropomorphic desires were attributed to the planets. Their motions were considered to arise purely mechanically, by the action of push contact forces.

When Newton made it clear in *Principia* that his aim was not to provide a mechanical explanation of celestial forces, proponents of the mechanical philosophy were understandably perplexed. Their bewilderment only grew when Newton began to refer to "attractions" and to the "tendencies" of non-contiguous bodies to attract one another, for such terminology smacked of old-fashioned Aristotelianism.

One aspect of Newton's work which the mechanical philosophers could not explain, of course, was the "beautiful discoveries" Newton was able to make with the help of his dubious non-mechanical conceptions. With his gravitational law Newton was able to demonstrate a multitude of disparate-seeming phenomena. This impressive ability set Newtonian forces apart from quantities normally regarded as occult, but only added to the confusion, among mechanical philosophers, surrounding the metaphysical status of Newton's dynamical conceptions.

Newton, in fact, shared with mechanical philosophers a disdain for occult quantities.[12] His response to the many charges of occultism laid against his dynamical conceptions was to insist that these conceptions were merely "mathematical", and certainly not intended to attribute to bodies any hidden powers.[13] Their purpose, according to Newton, was merely to quantify the forces acting, not to explain the origin of those forces, whether this origin be occult, mechanical or otherwise.

[12]Evidence for this attitude of Newton's can be found in many of his writings. According to Newton "[t]o tell us that every species of things is endowed with an occult specific quantity by which it acts and produces manifest effects is to tell us nothing ... " Thayer [76], pp. 176–177.

[13]For example, Newton insists explicitly on the merely mathematical character of his dynamical conceptions in the explication to Def. VIII, Book I of *Principia*, and in the opening and closing passages of Section XI of the same book.

From a modern perspective Newton's insistence on the mathematical, non-mechanical character of his dynamical conceptions looks positivistic, and one might conclude from this appearance that Newton held an accordingly anti-realist view of those conceptions, and towards gravitational forces in particular. Yet, there is good reason to believe this is not the case. Newton distinguished between mathematical and physical (i.e. mechanical) force, and there is evidence to suggest that he did not rule out the possibility that someone might yet discover the real physical mechanism which lay behind mathematical gravitational force.[14]

More importantly, Newton used his test theory in a way which undercuts the positivists' philosophical basis for anti-realism. Anti-realism arises from the belief that theory is underdetermined by phenomena—a belief which has its origin in either reductionist or hypothetico-deductive conceptions of theory-evidence relations (see Chapter 5). However, Newton does not share with the positivists their methodological conceptions, and he used his test theory to show how the character of celestial forces may in fact be overdetermined by observations. Newton overdetermines the inverse-square character of the Sun-directed force acting on the planets, by applying his test theory both to the quiescence of planetary perihelia and to Kepler's Third Law. Thus, Newton's application of his test theory steers us away, I believe, from ascribing to him a positivistic conception of 'force'.

What is more, the nature of his methods convinced Newton himself that the charges of occultism layed against his dynamical conceptions were misguided. For, by his methods, and by his use of a test theory in particular, Newton showed how one could derive forces from phenomena, and in Newton's view, that which can be derived from experience should not be regarded as in any way hidden.[15] In fact, Newton distinguished his conception of 'force' from both occult and mechanical conceptions. His forces were not occult, because they could be derived from phenomena. They were not mechanical, because they were abstract: according to Newton, they described merely quantitative, rather than qualitative, relations between physical bodies.

If Newton's view that forces describe merely quantitative relations looks strange today, it is because we no longer distinguish, as he did, between mathematical and mechanical force. For us, Newton's law of gravity is at the same time mathematical and mechanical. Its inverse-square form specifies the quality and not merely the quantity of gravity. It has never been shown, in fact, that a Cartesian-type mechanical reduction of Newton's dynamical conceptions is in any way possible. In my view, this fact suggests that, in one important sense of 'real', Newtonian forces should not be interpreted

[14]See, for example, Westfall [84], p. 509, or Hall [36], pp. 153ff, who both discuss Newton's late "return to the aether". Westfall emphasises, however, for what it is worth, that Newton's explanations of gravity were not always strictly mechanical. In particular, the last that he used actually posits actions at a distance among parts of the aether.

[15]See my quotation of Newton on Page 28.

realistically. We should not, that is, regard Newtonian forces as referring to concrete physical mechanisms or material things responsible for physical interactions. Nevertheless, the capability of Newton's test-theory method to determine, and even to overdetermine, these forces, suggests that we ought to regard Newtonian forces as referring to real causal relations which obtain between material things.

2.7 Newton's test theory and his experimental philosophy

I have argued that Newton had in his possession a test theory, and that he made important use of it in his argument for universal gravitation. My aim in this section is to clarify the historical context and philosophical motivation for Newton's test theory. I wish especially to show that this test theory was the product of Newton's development of a new and fruitful approach to science, an approach Newton himself called "experimental philosophy". In showing this, I relate Newton's test theory to further, very general, but still explicit features of Newton's practice, that is, to his methods of "deduction from phenomena" and "rendering general by induction", and to his even more general "method of analysis" and "method of composition".

In both the present section and the next I begin to examine the philosophical significance of the test-theory method. The success of Newton's and other test theories, I maintain, warrants the philosophical viewpoint presupposed by the test-theory method, which is the viewpoint of Newton's experimental philosophy. I shall argue here that Newton's methodology enabled him to set new standards for theoretical knowledge which surpassed the hypothetico-deductive conceptions he received from the mechanical philosophers. I shall argue later that contemporary physicists, by practising the test-theory method, have by and large maintained this standard in a different theoretical context. I shall contend, nevertheless, that Newton, in his understanding and employment of the method as a method of discovery, has something still to teach physicists today.

Newton began his scientific career as a mechanical philosopher, under the influence of figures like Gassendi and Descartes. The aim of mechanical philosophy, as I pointed out in Section 2.6, was to explain natural phenomena in terms of microscopic particles in motion. The explanatory hypotheses of mechanical philosophy, which referred to such particles, were to be developed speculatively, but with the important proviso that they describe intelligible mechanisms. Mechanical hypotheses were not to invoke mysterious powers or brute properties of matter as the ancients had done.

It was by adopting the mechanistic programme, and attempting to conduct fruitful scientific inquiry within it, that Newton acquired both an understanding of some of the programme's strengths but also a deep appreciation of its weaknesses. For him, the main problem with mechanical philosophy was not so much its aim as its method. While Newton accepted the mechanical philosophers' assessment of occult quantities as non-explanatory, he discovered that the speculative approach of Descartes and his followers too often gave rise to conceptions and theories which contradicted even gross features of experience. Newton believed that the existence of such contradictions itself contradicted the professed aim of mechanical philosophy, which was to explain natural phenomena by reference solely to particles in motion. Descartes' physical theories referred to moving particles all right, but cogent explanations of phenomena were lacking, and indeed were not possible given Descartes' theoretical suppositions.

Newton rejected, for example, the modification theory of colours (which many natural philosophers sought to render intelligible), for the reason that it ascribed to light properties which contradicted properties he had observed on many occasions in his own laboratory. Newton rejected the aethereal vortex by showing that it could not possibly give rise to celestial motions, as described by Kepler's Laws.[16] Newton also rejected Descartes' passive conception of matter (which underwrote his aethereal vortex theory), on the basis that it failed to account for the most obvious properties of material bodies, such as their inertia and impenetrability.[17]

Newton's reaction to his negative findings was not to hypothesise alternative mechanisms to explain phenomena which Descartes' theories could not explain. For that would have been to invite the same problems which beset the mechanical theories he had already rejected. Rather, Newton sought a surer way of constructing theories, a way which would guarantee, as far as possible, that one could use physical theories successfully to explain natural phenomena. Newton's belief that the physical theories should be derived from experiment and observation, rather than advanced speculatively, according to what seems intelligible to reason, formed the basis of Newton's new approach to physics, what he called "experimental philosophy".

In a letter to Roger Cotes, Newton explains how, in experimental philosophy, theoretical principles

> ... are deduced from phenomena and made general by induction, which is the highest evidence that a proposition can have in this philosophy. And the word 'hypothesis' is here used by me to signify only such a proposition as is not a phenomenon nor deduced from any phenomena, but assumed or supposed—without any experimental proof ... "For anything which is not deduced from phenomena ought to be called a hypothesis, and hypotheses of this kind, whether metaphysical or physical, whether of occult quantities or mechanical, have no place in experimental philosophy. In this philosophy, propositions are deduced from phenomena, and afterward made general by induction".[18]

[16]Newton's attack on the Cartesian vortical theory is contained in Section IX, Book II of *Principia*. Clifford Truesdell [79] maintains that the whole of Book II was written by Newton originally with the primary aim of overturning the vortical theory.

[17]Newton's devastating critique of Descartes' faulty understanding of matter is contained in his early, incomplete essay *On the gravity and equilibrium of fluids* (c. 1668).

[18]A fuller quotation is given by Thayer [76], p. 6.

Here, Newton explicitly distinguishes the speculative, or hypothetical, method of mechanical philosophy (and of the ancients) from his own more empirical approach to constructing theories. Newton's procedure for ensuring, as far as possible, that physical theories will agree with experience is to first deduce them from phenomena, and then render them general by induction. Newton states, moreover, that he regards this procedure as capable of delivering "the highest evidence" a theory can have in his philosophy.

In his *Opticks* Newton refers to this two-stage procedure singly as the "method of analysis" by which one "may proceed from compounds to ingredients and from motions to the forces producing them, and in general from effects to their causes and from particular causes to more general ones, till the argument end in the most general". [19] Newton contrasts the method of analysis with the "method of composition" which "consists in assuming the causes discovered and established as principles, and by them explaining the phenomena proceeding from them and proving the explanations". Newton further insists that "the method of analysis ought ever to precede the method of composition." One cannot hope for success, Newton claims, in theoretically predicting and explaining phenomena if one has not already derived one's theories from phenomena. Thus, Newton's methodological lesson is: *the method of analysis makes the method of composition possible.*

The methods of analysis and composition are precisely the ones Newton applies in *Principia*, a fact which he makes explicit in the preface to the first edition, where he promises in Book III to

> ... derive from the celestial phenomena the forces of gravity with which bodies tend to the sun and the several planets. Then from these forces, by other propositions which are also mathematical, I deduce the motions of the planets, the comets, the moon, and the sea.

Newton's explicit descriptions of his manner of proceeding, and the fact these descriptions so accurately conform to the actual structure of his arguments, show that Newton was well aware of the kind of methods he was employing.

Indeed, Newton's descriptions make clear for us what the overall methodological context and function of his test theory is. In the narrowest sense, Newton's application of his test theory in *Principia* is a striking example of his method of "deduction from phenomena". For, as I explained in Section 2.5, Newton used his test theory to deduce from celestial phenomena the inverse-square character of various forces acting in the Solar System. This test theory enabled Newton to determine empirically important physical attributes of specific physical systems, attributes which Newton later inductively generalised by means of his Rules of Reasoning. As deductions from phenomena, then, Newton's applications of his test theory form part of the crucial first step of his "method of analysis", by which he derives from motions the general causes which generate them. Now the ultimate purpose

[19] *Opticks*, Query 31, or see Thayer [76], pp. 178–179.

of the method of analysis is to make possible the method of composition, which is the method by which natural phenomena are explained. Hence, in the widest sense the methodological function of Newton's test theory is to ensure that the physical theories we develop explain at least what they are intended to explain. For Newton, then, his method of deduction from phenomena, and more specifically his implicitly held test-theory method, was a method for discovering explanatory theories.

With the help of his test theory Newton was able to discover what other mechanical philosophers only dreamed of. He was able to establish a simple, but perfectly general, physical principle (the law of gravity) which could be used to explain a vast number of natural phenomena. He was, thereby, able to show how disparate seeming celestial phenomena were systematically connected to one another. Of course, the abstractness of Newton's principle, and of his dynamical conceptions generally, worried mechanical philosophers, as I pointed out in Section 2.6. In order to achieve the result that he did, Newton needed to forgo the rationalist demand that physics concern itself first of all with the construction of intelligible theoretical mechanisms, and focus mainly on establishing strong connections between theory and phenomena. Evidently, Newton's test theory was an important tool for establishing such connections. Indeed, with his test theory Newton gave to the proposition of universal gravitation the highest evidence possible in his experimental philosophy.

2.8 The priority dispute with Hooke

An important gauge of Newton's advance beyond the speculative practices of contemporary natural philosophers was the occurrence of a priority dispute over the discovery of universal gravitation.[20] Given that Newton's test theory enabled him to deliver, for the first time, an especially cogent demonstration of the inverse-square character of celestial forces, it is perhaps surprising that this element of Newton's gravity theory, rather than any other, was the subject of a priority dispute.

However, I will contend that this priority dispute can readily, and advantageously, be understood in light of Newton's rapidly evolving approach to doing natural philosophy, relative to the approaches of his contemporaries, and relative to Hooke's understanding of scientific method in particular. This treatment, I will argue, makes possible a sympathetic appreciation of the claims of both scientists, while still ultimately awarding the discovery

[20] Another gauge is Huygen's *Treatise on Light*. In the preface to this treatise, which was written in 1690, more than two years after the publication of *Principia*, Huygens states that in natural philosophy "principles are tested by the inferences which are derived from them. The nature of the subject permits of no other treatment ... " (See Matthews [53], p. 126.) Evidently, Huygens had not yet come to terms with Newton's methodological innovations, for Newton had shown in *Principia* that the subject does indeed permit of another treatment, whereby principles can be "tested", indeed discovered, by deriving them from phenomena.

to Newton. An important consequence of looking at the priority dispute in this way, is that its deeper significance (and also the significance of Newton's test theory) for theoretical discovery in physics becomes clear.

The dispute in question arose before the first edition of *Principia* went to press. Hooke had, in 1686, got wind of Newton's plan to publish a new treatise, which included demonstrations of the celestial motions from the sole supposition of an inverse-square law of gravitation. This news provoked from Hooke (in Halley's words) "pretensions upon the invention of the rule of decrease of Gravity, being reciprocally as the squares of ye distances from the Center". [21]

The basis of Hooke's claim went back to an exchange between himself and Newton, which had occurred in 1679. At that time, Hooke had asked for Newton's opinion of his hypothesis that planetary motions are composed of a tangential motion together with "an attractive motion towards the centrall body ... "[22] During the ensuing correspondence Hooke suggested explicitly that the powers responsible for the attractive motion were of an inverse-square character.[23] However, the kinematical and dynamical principles from which Hooke had based his inverse-square law relation were false, and, I suspect, incoherent. Hooke had arrived at his conclusion by pure luck.

In 1686 Newton, after some deliberation, reacted negatively to Hooke's priority claim:

> But grant I received it [the inverse-square relation] ... from Mr Hook, yet have I as great a right to it as to ye Ellipse. For as Kepler knew ye Orb to be not circular but oval & guest it to be Elliptical, so Mr Hook without knowing what I have found out since his letters to me, can know no more but that ye proportion was duplicate *qua proxime* at great distances from ye earth, & only guest it to be so accurately & guest amiss in extending ye proportion down to ye very center, whereas Kepler guest right at ye Ellipse ... And so in stating this business I do pretend to have done as much for ye proportion as for ye Ellipse & to have a right to ye one from Mr Hook & all men as to ye other from Kepler.[24]

Newton's main charge was that Hooke's proposal of an inverse-square relation, like Kepler's proposal of elliptical orbits, was merely conjectural. It lacked the solid empirical basis which Newton had provided, with the aid of his test theory, for the very first time. Evidently, Newton was even prepared to grant, for the sake of argument, that he had obtained the inverse-square relation from Hooke, but refuses, nonetheless, to yield to Hooke the discovery of this relation. Hooke himself was no orthodox mechanical philosopher. That much is certain from his willingness to countenance the concept of 'attraction', which was anathema to the likes of Descartes and Huygens.

[21] Westfall [84], p. 446.
[22] Westfall [84], p. 382.
[23] Westfall [84], p. 386.
[24] From a letter to Halley of 1686. See Wilson [89], p. 90.

However, what Hooke had in common with mechanical philosophers, was their speculative-hypothetical approach to theory discovery.

Modern hypothetico-deductivists have said some strange things about theory discovery, things which are entirely out of step with our everyday understanding of the concept 'discovery', but which do, I think, help us to understand Hooke's priority claim on the inverse-square relation. The strange things hypothetico-deductivists have said about discovery derive ultimately, I suspect, from the dual nature of hypothetico-deductivism, that is, from its being at once hypothetical and empirical. One of the strange things they have said is that the discovery and justification of a scientific theory are to be strongly distinguished from one another. But since, according to hypothetico-deductivism, the justification of a theory concerns the empirical confirmation of its predictions, discovery is pushed back, perversely, to the hypothetical, or conjectural, stage of the scientific process.

The source of this bizarre distinction between the context of discovery and the context of justification is not hard to find. For, if one attempts to tie the discovery of theory to the successful prediction of phenomena, one runs into a seemingly insoluble demarcation problem. At what point in the context of justification are we to consider a theory discovered? After the first successful prediction? Surely not. But when, then? How many successful predictions are required? Any theory of broad application, such as those theories one typically finds in physics, will always have many more unconfirmed than it has confirmed predictions. So, if hypothetico-deductivism is the correct methodology, it seems that one cannot, without arbitrariness, link the discovery of a theory with its empirical confirmation. One is forced to link it instead with the theory's conjecture. That, or forgo talk of theory discovery altogether.[25]

Now the letter in which Halley first informed Newton of Hooke's pretensions on the inverse-square relation contains a notice that Hooke had conceded to Newton "the Demonstration of curves generated thereby", that is, the prediction of planetary orbits from the inverse-square relation.[26] This concession to Newton suggests that Hooke did not link the discovery of the inverse-square relation with a demonstration of its true empirical consequences.

It is therefore plausible, I believe, that Hooke, like modern hypothetico-deductivists, linked the discovery of the inverse-square relation to its invention rather than to its empirical confirmation. Thus, from Hooke's methodological perspective his priority claim on the inverse-square relation seems justified (assuming, of course, that Hooke really was the first to advance the "hypothesis" of an inverse-square relation). From Newton's perspective, however, Hooke's claim was completely unfounded. For, Newton had discovered that the hypothetical method of constructing theories was no longer the

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[25] Alan Musgrave [55], for example, contrasts the context of justification not with the context of discovery but with the context of invention.

[26] Westfall [84], p. 446.

most reliable method—indeed, in Newton's experience, it very often yielded unworkable results. From the point of view of his new experimental philosophy, the products of rational speculation looked like mere dreams. In particular, Hooke's original proposal of an inverse-square relation looked like a mere dream. Those who were sensitive to Newton's new approach agreed. In all probability Newton, fired with the passion of discovery, and incensed at Hooke's audacity, forgot that the methodological position occupied by Hooke once used to be his own, or at least something like his own. Now, Hooke's priority claim seemed to him not merely impertinent, but irrational, and he would denounce it as such.

In hindsight, we can see that Newton, and not Hooke, should be credited with the discovery of the inverse-square relation. The Eighteenth Century French scientist Clairaut, in support of Newton's new experimental philosophy, said of Hooke's examples that they "serve to show what a distance there is between a truth that is glimpsed and a truth that is demonstrated."[27] Yet we can also understand, I think, Hooke's reason for making a priority claim. Rather than merely as a desperate attempt to secure fame, Hooke's claim can be viewed more profitably as the result of his adherence to widely accepted epistemological doctrines which Newton had now surpassed.

Richard Westfall's conclusion, that

> [t]he interest in the incident [of priority] does not attach to the light it casts on the discovery of universal gravitation. It casts none. The discovery was Newton's, and no informed person seriously questions it.[28]

is, I believe, too glib. In my view, the priority dispute is very illuminating of the discovery of universal gravitation, and indeed of discovery in general. For, what was in dispute between Newton and Hooke was not merely the discovery of a natural law, but the very nature of scientific discovery itself. Newton's test theory, and his constructive, non-speculative method of deduction from phenomena generally, enabled Newton to set new standards for discovery—standards which in 1686 only he had positively grasped.

[27]Westfall [84], p. 452.
[28]Westfall [84], p. 451.

CHAPTER 3

Modern test theories and Einsteinian physics

The demise of Newtonian physics, and the rise and development of relativity theory, has proved of enduring interest to philosophers who study the theory of knowledge. Yet investigation by philosophers of the role of test theories in this development has until recently been nonexistent, to the detriment, I believe, of our understanding of relativity theory as a preeminent example of scientific knowledge.

Tensions existing in nineteenth-century physics motivated Einstein to advance beyond the Newtonian world picture by revising certain fundamental beliefs about the nature of time and space. It is commonly held that in making these revisions Einstein was influenced much less by specific phenomena (such as the anomalous perihelion shift of Mercury, and results of the Michelson-Morley experiment) than by more general theoretical considerations. However, this view has been challenged recently by some philosophers who argue that Einstein in fact employed Newton's method of deduction from phenomena to advance beyond Newtonian physics.[1]

I will endorse, in this chapter, the view that Einstein employed Newton's method of deduction from phenomena, and I will argue, moreover, that Einstein's example has motivated contemporary physicists to construct test theories for relativity. In so doing, I will show that contemporary physicists have, like Newton, consciously formulated methodological frameworks in which are parameterised a range of alternative physical theories, and I will indicate how physicists have used these frameworks empirically to peg down a good many elements of Einstein's relativity theories. I will argue, on the basis of these developments, that Einstein's theories, despite their patently abstract and somewhat counter intuitive character, are after all preeminent examples of scientific knowledge—that is, of knowledge which profoundly unifies, but which is also strongly conditioned by, experience.

I will conclude that contemporary physicists have maintained the standards for scientific knowledge originally set by Newton, i.e. that physicists today have given to relativity theory "the highest evidence" possible in experimental philosophy. In addition, I will conclude that important elements of Newton's practice, particularly those connected with his use of a test the-

[1]Dorling [15]; Catton [12].

ory, have not only passed into the reflexive practice of modern physics, but now form part of its conscious method. They are part of how present day physicists are taught to work.

In Section 3.1 I discuss how the development of field theory in nineteenth century physics encouraged a return to Cartesian-type mechanistic thoughtways, and I discuss how this return introduced tensions into the Newtonian world view. Nevertheless, in Section 3.2 I provide a simple example of a test theory for a classical, non-relativistic field, thereby demonstrating that test theories for fields are possible. In Section 3.3 I describe how Einstein resolved the tensions introduced by field theory into physics by first seeing his way clear of the resurgent Cartesian thoughtways, and then by using Newtonian methods to advance beyond Newtonian theory. In Section 3.4 I indicate how Einstein's Newtonian-styled derivation of the Lorentz transformation may be modified to produce a simple, but philosophically significant, test theory for relativistic kinematics. In Section 3.5 I look at the pioneering test theory for relativistic kinematics due to H. P. Robertson. I examine this test theory's special features and Robertson's motivation for its construction. In Section 3.6 I describe Eddington's early test theory for relativistic gravity. In Section 3.7 I present what is perhaps the pinnacle of modern test theories, the Parametrized Post-Newtonian (PPN) Formalism for relativistic gravitational fields. In Section 3.8 I look at an important application of the PPN Formalism: the search for gravitomagnetism. In Section 3.9 I discuss briefly the role of modern test theories in the progress of physics towards a more unified theory of the fundamental interactions. Finally, in Section 3.10 I summarise the achievements of modern test theories, and I review some of the factors which made these test theories possible .

3.1 Field theory and neo-Cartesian mechanical philosophy

Faraday's introduction of fields into nineteenth-century physics acted to weaken the dominance of Newton's mechanical principles, and eventually brought about the conditions for their demise.[2] However, this ultimately successful challenge to Newtonian physics did not in the end undermine Newton's experimental philosophy. On the contrary, the downfall of Newtonian principles in fact strengthened experimental philosophy. One aim of the present chapter is to show this, and to remove any suggestion of paradox concerning it. I will describe in this section, and in Section 3.3, how Einstein acquired a Newtonian, anti-mechanistic attitude towards physical fields, and I will argue that this newly acquired attitude actually made possible Einstein's advance beyond Newtonian physics. Ultimately, I will contend that it was Einstein's implicit rejection of resurgent Cartesian thoughtways and "return" to Newtonian experimental philosophy that brought about philosophical conditions favourable for the later development of test theories for relativity.

[2]This view is supported by Einstein and Infeld [24], pp. 71–185.

Newton, recall, had made significant advances in physics by letting go the rationalist demand that physics concern itself first of all with constructing intelligible mechanisms for the explanation of natural phenomena. Bare demands of logical coherency aside, Newton's new experimental philosophy required only that a strong connection be established between phenomena and theory. On this basis, for example, Newton abandoned the superficially intelligible, but empirically unsatisfactory, aethereal vortex theory of Descartes, replacing it with his own relatively abstract, but empirically well-grounded, theory of gravitational force.

Yet the success of Newton's experimental philosophy did not act entirely to banish from physics old-style, Cartesian, proclivities to intelligible mechanistic explanation. In the eighteenth century, scientists interested in electrical and magnetic effects believed these effects to be transmitted by subtle fluids—the so-called electric and magnetic ethers. Some scientists also posited an ether to explain gravitational attraction. At the beginning of the nineteenth century Young and Fresnel resurrected the wave theory of light, and referred to a luminiferous ether as the medium for light propagation. It is true that the early mathematical development of electromagnetic theory, by Poisson, Green and Gauss in the first half of the nineteenth century, assumed action at a distance. However, Faraday's introduction (between 1845 and 1850) of the ideas of 'contiguous magnetic action' and 'lines of force', to describe the spatial locality of certain electromagnetic phenomena, seriously threatened the viability of that assumption.

Faraday's concepts we now refer to under the single heading 'fields'. Fields are continuous functions of time and position. Their evolution and spatial configuration are governed by partial differential equations, called field (and sometimes wave) equations. Mathematically, the concepts of 'field' and Newtonian action-at-a-distance 'force' are equally abstract. Yet the continuous character of fields, their ability to fill space, suggests Cartesian aethereal mechanisms, and therefore action by contact. Furthermore, the solutions of field equations typically describe fields as propagating transverse waves. Familiar examples of transverse waves, such as those on water and strings, require a medium through which to propagate. By analogy, the suggestion was, in the nineteenth century, that electromagnetic field waves also required some kind of elastic, space-filling medium—an ether—through which to propagate.

Of course the mere appearance of fields in a physical theory is not alone sufficient to imply contiguous action in, or the propagation of a physical something through, space. In Section 3.2 I will point out how the modern field-theoretic formulation of Newton's gravity theory does not, in contrast to most other field theories, contain a dynamical, energy-bearing field which can propagate independently of its source—a fact linked intimately with Newtonian gravity's action-at-a-distance character. The implication is that the Newtonian gravitational field has a somewhat fictitious nature.

In contrast, Maxwell's theory of electromagnetism does contain dynamic electric and magnetic potential fields which carry energy and momentum. Maxwell demonstrated that such fields could in fact propagate independently of their source, and that they would do so with a very definite finite velocity, the velocity of light. These results suggested not only that the electric and magnetic fields (unlike the Newtonian gravitational field) referred to real physical structures, but also that light itself was composed of electromagnetic waves.[3]

However, Maxwell's field equations look markedly different from Newton's laws of motion. The former are partial differential equations for continuous fields, whereas the latter are ordinary differential equations for forces acting between discrete matter. Nineteenth-century physicists sought to bring greater coherence to physics by trying to provide a mechanical foundation for Maxwell's equations, that is, by seeking to interpret Maxwell's equations in terms of Newton's laws. Manifestly, Maxwell's equations are not directly reducible to Newton's laws because of their very different character, so attempts were made to give a mechanical description of the substratum, the ether, which allegedly supported the transmission of electromagnetic waves.

No such mechanical description emerged which could properly unify the phenomena, however. The phenomena suggested that electromagnetic waves can be of high or low frequency. But to treat low frequency waves as mechanical vibrations the ether needed to be rather elastic, whereas to treat high frequency waves in this way it needed to be very rigid. In addition, stellar observations indicating that the ether was not dragged around by the movement of matter implied that the ether is non-resistive, whereas the null results of Michelson and Morley indicated no ether drift (relative motion of ether to earth).

Attempts by Fitzgerald and especially Lorentz to resolve these and similar paradoxes produced important mathematical results (e.g. the Lorentz transformations), but did not ultimately overcome the ether's lack of adaptability to the facts. Such was the failure to construct a mechanical ether consistent with all known phenomena that many physicists sought to replace this very programme with a contrary one in which the aim was to reduce mechanics to electromagnetism. According to this programme, the inertia of material particles was to be explained as a resistivity to acceleration of certain discontinuities in the electromagnetic field. Indeed, everything in physics was to owe its origin to the evolution of an electromagnetic field in accordance with Maxwell's laws.

Thus, the introduction of field conceptions into nineteenth-century physics directly challenged not only the applicability, but also the fundamentality, of Newton's physical conceptions. Fields described natural phenomena in terms of continuous quantities acting contiguously, rather than in terms of discrete material particles acting on one another at a distance. Because of fields nineteenth-century physicists came to distinguish strongly between

[3]Einstein and Infeld [24], pp. 148–156.

different types of phenomena (e.g. between electromagnetic and gravitational phenomena), creating conceptual disunity. Initial efforts to overcome this disunity, and reassert the primacy of Newtonian conceptions, aimed at providing a mechanical interpretation of Maxwellian electromagnetism, but succeeded only in compounding the tensions that already existed between the force- and field-theoretic descriptions of nature. Further attempts to reconcile the differing conceptions aimed instead at reducing forces to fields. However, the first significant step towards such a reconciliation came from a different direction. This step was taken by Einstein in 1905.

3.2 Aside: A test theory for the Newtonian gravitational field

Newton's test theory presupposes a particular dynamical framework—the framework consisting of his three Laws of Motion. Although this framework is sufficient for the construction of test theories, I will show in this section that it is not necessary for their construction. I will show this by formulating a test theory for Newtonian gravity not in terms of forces, but in terms of fields, thereby paving the way for a discussion of modern test theories, which also do not presuppose Newton's force-theoretic conceptions. I will begin by exhibiting the correspondence between the field- and force-theoretic formulations of Newton's gravity theory. Then, I will confirm that the field-theoretic formulation does in fact respect action at a distance, despite its use of a continuous field. Finally, I will show how one can construct a test theory for the Newtonian gravitational field, by making arbitrary, or parameterising, certain elements of the field-theoretic formulation, but without violating essential Newtonian features like action at a distance.

The source of Newtonian gravity is the Galilean-invariant mass M. The simplest field theory for an interaction with M as its source has the following field equation (valid in inertial frames):

$$\boldsymbol{\nabla}^2\Phi = 4\pi G\rho \ , \tag{3.1}$$

where $\Phi(t, \mathbf{x})$ is a proper 3-scalar potential field generated by the proper mass density $\rho(t, \mathbf{x}) = dM/d^3x$, which describes the distribution of matter throughout space. That this field equation is the simplest possible is evident from the fact that it has only a single term (apart from the source term) and that the field appearing in this term is of the most rudimentary type, a scalar.

The equation of motion for bodies, of masses m_i, moving along trajectories $\mathbf{z}_i(t)$ under the influence of Φ is:

$$m_i\ddot{\mathbf{z}}_i = -m_i\boldsymbol{\nabla}\Phi(t, \mathbf{z}_i) \ . \tag{3.2}$$

Correspondence between Equations 3.1 and 3.2 and the force-theoretic formulation of Newtonian gravity can be exhibited by (1) making the identification $\rho = \sum_i M_i\delta(\mathbf{x} - \mathbf{z}_i)$, i.e., by assuming that the matter density distribution consists of countably many point particles, and (2) recognising the left hand side of Equation 3.2 as being the Newtonian force $\mathbf{f}_i$. Integrating Equation 3.1 then yields

$$\mathbf{f}_i(t) = -G \sum_{j \neq i} m_i M_j \frac{\mathbf{z}_i(t) - \mathbf{z}_j(t)}{|\mathbf{z}_i(t) - \mathbf{z}_j(t)|^3} , \tag{3.3}$$

which is Newton's inverse-square law of gravity.

Evidently, Equation 3.3 respects action at a distance, for the $\mathbf{f}_i$ are not functions of the particle locations $\mathbf{z}_i$ at different times, but only of differences in locations at the *same* time $t_i = t_j = t$. The forces $\mathbf{f}_i$ act instantaneously across space. This result must obtain in any theory which presupposes Galilean spacetime, and in which force is a function of distance, since the distance between spatial points lying on different planes of simultaneity is not defined in Galilean spacetime.[4]

At first glance, it may seem that the field-theoretic formulation of Newtonian gravity violates action at a distance. For, by Equation 3.2, bodies subject to the gravitational field Φ receive their marching orders, as it were, not from a distance but from the local structure of Φ, that is, from $\nabla\Phi$. However, according to Equation 3.1, Φ is not a dynamical field.[5] No time derivatives of Φ appear, which means that the solutions of Equation 3.1 do not describe propagating waves. The spatial configuration of Φ is globally and instantaneously conditioned by the distribution of matter ρ. There is no suggestion of contiguous action occurring between any part of the field and its neighbouring parts. Hence, the field-theoretic formulation of Newtonian gravity respects action at a distance.

A test theory for the Newtonian gravitational field Φ can be constructed simply by adding terms containing free parameters to Equation 3.1. These extra terms should not, of course, bring about a violation of principles essential to the Newtonian world view, such as parity conservation and Galilean relativity (which implies action at a distance). Hence, they will need to be proper scalar quantities, with no time derivatives appearing. If we also restrict our attention to a second-order equation, then only two extra terms can be added. One of these terms is linear in Φ, the other is a time constant. In this case Equation 3.1 becomes:

$$(\nabla^2 + \frac{1}{\lambda^2})\Phi + \Lambda = 4\pi G\rho . \tag{3.4}$$

[4]Classical theories with retarded potentials, such as classical electrodynamics, appear to contradict this maxim. However, such theories strictly require the distance between different planes of simultaneity to be defined, since the potentials are functions of the difference in particle locations at *different* times. Hence, such theories require a frame of absolute rest. They require an ether or, what amounts to the same thing, absolute space, not relativised space as in Galilean relativity. For further discussion, see Friedman [29], p. 105.

[5]Michael Friedman [29] (p. 94) errs when he calls Φ a dynamical object. He also errs when he claims the dynamical character of ρ is evident from the field equations of NGT. In fact, the dynamical character of ρ derives, indirectly, from the equation of motion, Equation 3.2, not from the field equations.

Evidently, the parameter λ is a real length, while Λ is a function of time of dimension $[\text{time}]^{-2}$. No fundamental length or time constants can be derived from basic Newtonian principles, so these two parameters need to be fixed empirically. Equation 3.4 constitutes the conceptual basis of a two-parameter test theory for the Newtonian gravitational field which I shall now examine.[6]

For the sake of clarity I will treat our two test-theory parameters λ and Λ separately. Let us assume to begin with that $\lambda = \infty$ and consider (as did Newton) a physical system in which a body orbits circularly about a gravitating point. From Equations 3.4 and 3.2 we obtain

$$\Phi(R) = -\frac{GM}{R} - \frac{\Lambda R^2}{6} + \text{const.} \iff T \propto \frac{R^{3/2}}{(GM - \Lambda R^3/3)^{1/2}}, \qquad (3.5)$$

where R and T are the orbital radius and period, respectively. The equation on the left-hand side constitutes the basic equation of our test theory. The first term in this equation is the standard Newtonian potential giving rise to an inverse-square interaction. The second, "harmonic oscillator" term in Λ generates a new effect, the strength of which increases proportionally as the distance from the gravitating body.[7] Depending on whether Λ is positive or negative, the new effect is repulsive or attractive, respectively. The denominator in the expression on the right-hand side of Equation 3.5 indicates that the Λ-term makes a point source appear more (or less) massive than it really is. Although the Λ-term is independent of M we may nonetheless consider it a gravitational term, because it arises only in gravitational field equations. For the special case $\Lambda = 0$, we obtain, from the right hand side of Equation 3.5, the standard Keplerian relation $T \propto (R^3/GM)^{1/2}$.

If we consider now the same physical system, but assume instead that λ is finite and $\Lambda = 0$, then Equations 3.4 and 3.2 give

$$\Phi(R) = -\frac{GMe^{-R/\lambda}}{R} + \text{const.} \iff T \propto \frac{R^{3/2}e^{R/2\lambda}}{[GM(1 + R/\lambda)]^{1/2}}. \qquad (3.6)$$

In contrast to the Λ term, the λ term is dependent on the mass M of the source; and leads to a short- rather than long-range perturbation of the basic inverse-square interaction. For, when $R \gg \lambda$, that is, at large distances, $\Phi \approx -GM/R$, and $T \approx (R^3/GM)^{1/2}$. At shorter distances the λ term acts to make the point source appear more massive that it really is.

Just as phenomena serve to fix, or at least put bounds on, the value of the parameter n in Newton's test theory, so phenomena serve to fix the

[6] I am much indebted in various ways in this chapter to Noel Doughty [16]. Doughty derives Equation 3.4 from first principles on p. 138ff of his book.

[7] Einstein introduced a similar term into general relativity theory. Nowadays, such terms are referred to as cosmological constant terms, because their effects would be most evident on very large scales. Newton himself considered the possibility of a "harmonic oscillator" force in celestial dynamics. He argued that such a force was compatible with a harmonious universe (Westfall [84], pp. 440–441), but rejected it ultimately for empirical reasons, as we have seen.

values of the parameters Λ and λ in our test theory for the Newtonian gravitational field. Like n, Λ and λ concern the distance dependence of gravity, but they do so in perhaps a more revealing way. For in field theory the inverse-square interaction is the most basic one, arising from the simplest field equation of an interaction with mass as its source, whereas in Newton's force-theoretic analysis the inverse-square proportion has no such privileged theoretical status. Furthermore, in the field case, perturbations on the inverse-square proportion arise through contributions from other "forces", of a number and character heavily circumscribed by high level Newtonian principles like parity covariance and Galilean relativity. In contrast, Newton's test theory assumes that only one force is acting.

Had nineteenth-century physicists possessed a test theory for the Newtonian gravitational field, like the one I have presented in this section, they might have attempted to use it to analyse so-called anomalous precessions in the apsides of planetary orbits. However, the results of such an analysis would have disappointed them, for we now know that these precessions arise from Post-Newtonian relativistic terms of different character from the terms containing Λ and λ.[8] The known values of anomalous apsidal precession will give in fact conflicting values for each of these two parameters, meaning that the phenomena fail to pick out any one member of the family of theories parameterised by this test-theory construction.

In this section I have considered a test theory based on a scalar (rank 0) field equation of second order, this being the traditional and highly successful form of Newtonian gravity theory. Equation 3.4 represents the most modest perturbation on the orthodox Equation 3.1. To date, phenomena on galactic scales (in particular, the rotation rates of galaxies) suggest a possible contribution to gravity from the Λ term.[9] Phenomena on smaller scales, however, give no conclusive indication that λ is a finite quantity.[10] Higher order equations (containing tensors of higher rank), and hence more complex test theories, should also be possible in a Newtonian context. However, none of the theories represented by these test theories would constitute the classical limit of a realistic theory of relativistic gravitation.

3.3 Relativity theory and Einstein's neo-Newtonianism

In his 1905 paper "On a Heuristic Point of View Concerning the Production and Transformation of Light" Einstein took Planck's novel black body radiation law to imply that radiation exists only in discrete packets (quanta), not, as Maxwell's equations imply, in continuous waves. In advancing this interpretation Einstein made the first step towards reconciling the prevailing

[8] In fact, the "harmonic oscillator" term in Λ, like the inverse-square term, yields stationary apsides—a fact which is evident from Equation 2.3 (given that the gravitational potential is just the first integral of gravitational force divided by the mass).

[9] Mannheim [51].

[10] Will [88].

view of matter, as discrete, with the continuum field-theoretic view of light (and of radiation generally). Also in 1905 Einstein dealt with that "enfant terrible" of nineteenth-century field theory, the classical ether.[11] Einstein's solution to the problems which beset attempts to interpret Maxwell's equations mechanically was to abandon the classical ether altogether. This move necessitated revising a very deep element of theory—the classical concept of time.

In this section I argue that Einstein's revision, while concerned with a traditionally metaphysical subject (the nature of time), and while involving the rejection of a doctrine basic to Newtonian physics, nevertheless can be counted a triumph of Newtonian experimental philosophy. Accordingly, Einstein's revision can be counted a defeat for Cartesian mechanistic thoughtways which seemed to gain new life in the nineteenth century with the development of field theory. In my view, Einstein's superb derivation of special relativistic kinematics not only exhibits a Newtonian style of reasoning, as Jon Dorling and Philip Catton argue, but has also provided important stimulus for the subsequent construction of test theories for relativity.

The story of relativity theory begins with Maxwell's field equations. Maxwell had derived from his equations that electromagnetic waves propagate through space with constant speed $c = (\mu_0 \epsilon_0)^{-1/2}$. In the classical kinematical setting of nineteenth-century physics, this result implied that Maxwell's equations were valid only in a single inertial frame. For, according to the classical velocity addition law, electromagnetic waves observed travelling at c in inertial frame F would travel at $c - v$ in a frame F' moving at speed v relative to F. In the nineteenth century it was assumed that this frame was the rest frame of the ether which allegedly supported the propagation of electromagnetic waves.

Today, there is abundant evidence that Maxwell's equations hold good (for actions $\gg \hbar$) not just in a single inertial frame, but in all inertial frames. This result suggests that the classical ether, which defines a privileged rest frame, should be abandoned and that Galilean transformations should be replaced by those of Lorentz as the correct transformations linking inertial frames. Indeed, modern physics texts often motivate special relativity theory in this very way.

Einstein himself could have derived the Lorentz transformation from the full set of Maxwell's field equations. In fact, there is evidence that Einstein actually performed such a derivation.[12] However, Planck's black body radiation law likely convinced him that such a derivation would not be sound. Einstein's quantum interpretation of Planck's result conflicted with the continuum picture of radiation implied by Maxwell's equations. Consequently, Einstein regarded Maxwell's equations as only approximately true, and therefore probably thought them unfit as a sound basis from which

[11] Einstein and Infeld [24], p. 184.
[12] See Einstein [23] and Earman et al. [17].

to make further advances in physics.[13]

In any case, it appears that Einstein fastened onto an observable consequence of Maxwell's equations which should have been (and in fact was) independent of whether the electromagnetic interaction propagated as quanta or as continuous waves. This consequence was the constant speed c of electromagnetic propagation, and is normally referred to as the Light Postulate. In his 1905 paper "On the Electrodynamics of Moving Bodies", Einstein [22] took as premises the Light Postulate, a Relativity Principle, and also certain high-level innocuous background assumptions (concerning, for example, the homogeneity of space), and from these deduced the Lorentz transformation.

In the context of Einstein's derivation, the Lorentz transformation is no longer a transformation linking the privileged rest frame of the ether (associated with absolute time) with other inertial frames, as it had been for Lorentz. For, Einstein's Relativity Principle expresses an equivalence between all inertial frames. In adopting this principle Einstein had, in effect, abandoned the classical ether. Consequently, the Lorentz transformation was now to be regarded as the correct transformation linking together all inertial frames with one another. In this role, the Lorentz transformation had a profound implication for the nature of time. Specifically, it follows from Einstein's result that two events δx apart and observed to occur simultaneously in frame F ($\delta t = 0$), will be observed to occur successively ($\delta t' = v\delta x/c^2(1 - v^2/c^2)^{1/2} \neq 0$) in frame F' moving at speed v relative to F. Thus, Einstein's result implies that what events are to be counted simultaneous is no longer independent of an observer's trajectory. In choosing to jettison the classical ether Einstein was led, therefore, to revise the classical concept of absolute time.

In my view, Einstein's revision of this concept was clearly motivated by empirical concerns. Einstein was well aware of the problems in accounting for empirical facts facing the attempt to describe the ether mechanically. He argued (at the start of his 1905 relativity paper) that symmetries inherent in electromagnetic phenomena contradicted the view, maintained by supporters of the classical ether, that there exists a privileged state of rest. In addition, Einstein (probably) took Plank's phenomenological law to imply that Maxwell's set of equations were untrustworthy as basis from which to advance physics. Einstein did not look, as a metaphysician might look, to the conceptual presuppositions of accepted physical theory to discover what time was like. Instead, he derived the character of time from a phenomenon (constancy of light speed) that he regarded as robust together with certain background beliefs that he took to be innocuous.

These features of Einstein's derivation have led Jon Dorling [15] and Philip Catton [12] to conclude that Einstein in fact discovered special relativity theory by employing Newton's method of deduction from phenomena.[14]

[13]Evidence for Einstein's early dissatisfaction with Maxwell's equations can be found among recollections to Max von Laue, in a letter dated 17 January 1952.

[14]Whether or not Einstein used the method to argue also for general relativity, as Jon

For example, Dorling writes:

> To recognise it [i.e. Einstein's argument] as formally a Newtonian-styled deduction from phenomena, it is only necessary to recognize that it is the Lorentz transformations which must explain the constancy of the speed of light and not vice versa ... So Einstein really has derived an explanans from one of its own explananda in the classical Newtonian manner.[15]

Dorling and Catton's point is that Einstein advanced beyond Newtonian physics in a very Newtonian way. Thus, while Newtonian physics foundered, the very method which had previously assumed Newtonian dynamical principles for its application actually gained in support, by being instrumental in the correction of these same principles.

In my view, a precondition for Einstein's application of Newtonian methodology was his lack of commitment to, and awareness of the weaknesses in, the programme which sought to interpret Maxwell's equations mechanically. According to Einstein, this programme

> ... was zealously but fruitlessly attempted, while the equations were proving themselves fruitful in mounting degree. One got used to operating with these fields as independent substances without finding it necessary to give one's self an account of their mechanical nature ...[16]

Einstein learned to treat electric and magnetic fields as abstract mathematical quantities, in just the same way that Newton treated the forces appearing in his dynamical principles as abstract mathematical quantities. Newton had abandoned the demand that one must first give an intelligible mechanical reduction of the concept 'force' before one could feel confident in applying it. Einstein let go of the same demand with regard to fields. This weakened, I believe, his commitment to the classical ether, and provided philosophical motivation for the Relativity Principle, a crucial ingredient in his derivation of the Lorentz transformation.

It would be unfair, however, to characterise Einstein as merely reinventing the Newtonian philosophical wheel. For fields, much more than Newtonian forces, suggest the need for some kind of mechanical medium to support them, as I pointed out in Section 3.1. It is remarkable that anyone was able to see their way clear of this, ultimately illusory, need. In seeing his way clear Einstein gave Newtonian experimental philosophy an enormous boost, and at the same time dealt a heavy blow to resurgent Cartesian thoughtways. It is ironic, of course, that what nineteenth-century physicists would have regarded as a satisfactory mechanical interpretation of electromagnetism was not at all the kind of interpretation that would have

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Dorling [15] contends, it is clear is that Einstein could not have reasoned his way to curved spacetime by looking to the conceptual presuppositions of existing physical theory. For in 1915 no physical theory existed which presupposed curved spacetime.

[15]Dorling [15], pp. 5–7.

[16]Schlipp [73], pp. 25–27.

impressed Descartes. For what these physicists desired was a reduction of electromagnetism to "occult" Newtonian forces. By the nineteenth century, it was fields which, unsupported by a material substratum, seemed "occult" and unintelligible, while physicists had become used to operating with the abstract dynamical concepts of Newtonian physics.

In my view, it was Einstein's neo-Newtonian, anti-mechanistic attitude towards fields which brought about philosophical conditions favourable for his brilliant Newtonian-type demonstration of the need to move beyond Newtonian physics. Not only did Einstein's new attitude provide philosophical motivatation for the Relativity Principle, it made acceptable to him a more abstract, and considerably more counter intuitive, concession to the demand for locality than nineteenth-century physicists were seeking.[17] John Norton has emphasised the importance to the discovery of special relativity of Einstein's "disclosure and rejection" of the belief in the absolute character of time.[18] In my view, Einstein was able to let go of this highly intuitive belief about time, precisely because he was already thinking very abstractly about other, related physical conceptions.

The rigorous deductive character of Einstein's derivation from phenomena of the kinematics of special relativity theory has been a source of inspiration to many physicists attempting to provide cogent arguments for various elements of physical theory.[19] Some of the most explicit and most fruitful of these imitations have involved the construction of test theories for different parts of relativity theory. I will contend that, even more than Einstein's argument for special relativity, these test theories reflect Newtonian-style methods.

3.4 A test theory for relativistic kinematics

Test theories for relativity divide naturally into two groups: those for relativistic kinematics and those for relativistic gravity. Test theories in the first group include special relativity theory as a special case. Test theories in the second group include general relativity theory as a special case. While Einstein's argument for the Lorentz transformation appears to be a strik-

[17]Another interpretation of Einstein, one which at first glance seems at odds with my own interpretation, has been advanced by the philosopher Harvey Brown. Brown argues that it was always Einstein's intention ultimately to deliver spacetime principles from out of a theory of matter, and that his way of working in connection with relativity theory—aimed somewhat positivistically at the delivery of a "principle" rather than a "constructive" theory— was simply an expediency, because matter theory at that time was in too primitive a state to deliver what Einstein asked of it. Evaluating Brown's thesis is, of course, beyond the scope of this dissertation, but I do not regard his thesis as incompatible with my own. For my view is that both "positivist" and more "mechanistic", or "rationalist", ways of working have been important for the development of physics. See Section 4.9.

[18]Norton [61], p. 37.

[19]A physics text replete with such arguments is Noel Doughty's "Lagrangian Interaction" [16].

ing example of Newtonian deduction from phenomena and has, I believe, inspired the construction of test theories for relativity, the argument itself does not, strictly speaking, involve a test theory. The reason it does not is because its premises lead uniquely to one theory, special relativity, without any empirical fixing of free parameters.[20] Einstein's argument, in contrast to test-theory arguments, does not empirically select special relativity from out of a parameterised class of alternatives (though John Norton has argued that Einstein actually had in mind a large number of rival theories to special relativity which were "eliminated" by his derivation of the Lorentz transformation.[21])

Nevertheless, Einstein's argument may easily be modified so that it does involve a simple test theory. Instead of applying the Light Postulate from the outset, as Einstein did, it is possible to perform Einstein's derivation from the first postulate alone, that is, from the Relativity Principle.[22] This derivation yields the following transformation equations:

$$\begin{aligned} \tau &= \beta(t - \alpha v x) \ , \\ \xi &= \beta(x - vt) \ , \\ \eta &= y \ , \\ \zeta &= z \ , \end{aligned} \tag{3.7}$$

where

$$\beta = \frac{1}{\sqrt{1 - \alpha v^2}} \ . \tag{3.8}$$

These expressions are identical to those found in Einstein's paper "On the Electrodynamics of Moving Bodies" apart from the appearance of the free parameter α, which replaces Einstein's expression $1/V^2$ (where V is the speed of light). Because Equations 3.7 and 3.8 contain a free parameter to be fixed empirically, we can regard them, taken together, as the basic equation in a test theory for relativistic kinematics. From this equation we can derive expressions containing observable quantities, which we can then use to determine α empirically. Following Einstein, for example, we can derive an expression for the addition of speeds:

$$U = \frac{v + w}{1 + \alpha v w} \ . \tag{3.9}$$

There are three cases to consider, according to whether α is negative, zero or positive. A negative value will never obtain, for it leads to the physically absurd result that two velocities in the same direction may add to

[20]It is true that Einstein's argument does contain free parameters, but Einstein determines their values by recourse to various high-level theoretical assumptions (which, typically, have been carried over from Euclid's theory of space), rather than by appeal to phenomena.

[21]Norton [61], pp. 35–38. Norton characterises Einstein's argument as an eliminative induction, rather than as a Newtonian deduction from phenomena, as I have done.

[22]See Lee and Kalotas [46]; Lévy-Leblond [48]; Doughty [16].

give a velocity in the reverse direction. Equivalently, it yields transformation equations which predict causality violations.[23] If $\alpha = 0$, then Galilean relativity obtains. A positive value of α yields the Lorentz transformations.

Evidently, α has dimensions of $[\text{velocity}]^{-2}$. Because we are ignoring the case where α is negative, we can set $\alpha = 1/\sigma^2$, and since we require $\alpha v^2 < 1$, by Equation 3.8, we must have $0 \leq v < \sigma$. Hence, σ is a new universal constant connected with the idea of a limiting velocity. In fact, the derivation of Equations 3.7–3.9 shows that the very notion of a limiting velocity arises independently of any knowledge of the nature of light—it arises solely from the relativity principle, taken together with certain other innocuous assumptions concerning the character of space and time.

While α can in principle take on any positive value, as well as zero, the highly restrictive constraint of the relativity principle (and accompanying innocuous assumptions) produces a kinematic test theory with only two physically distinguished possibilities: Galilean relativity theory ($\sigma = \infty$), which underlies Newtonian physics, and special relativity theory ($0 < \sigma < \infty$). According to a multitude of phenomena (including, for instance, particle collisions in accelerators), the value of the ultimate speed σ is indistinguishable from the speed of light—a result which evidently rules out Galilean relativity theory as a universally viable kinematic theory.

Although Einstein did not employ the test theory I have presented here to discover special relativity theory, it is clear that he nearly did. For if he had simply applied the light postulate at the end of his derivation of the Lorentz transformation rather than at the beginning his argument would certainly have involved this test theory. The significance of this test theory for the philosophy of science derives from the fact that it represents both Galilean and Einsteinian kinematics within a single formal structure. Some philosophers of science believe that there is just no way to judge competing scientific theories independently on evidential grounds. However, our simple test theory shows that there is such a way to judge Einsteinian, as opposed to Newtonian, physics. In Section 5.6 I will use this fact to criticise Thomas Kuhn's doctrine of incommensurability.

The test theory for relativistic kinematics described in this section was obtained simply by omitting the Light Postulate from Einstein's derivation of the Lorentz transformation, while retaining the Relativity Postulate. A more complex test theory for relativistic kinematics can be obtained by doing the converse, that is, by retaining the Light Postulate and omitting the Relativity Postulate. This is precisely what H. P. Robertson did in 1949.

[23]This is true, at least, in the standard coordinates in which Equation 3.7 is expressed. Geoff Stedman has pointed out to me that in some coordinate systems (e.g. in some of the systems represented by Equation 4.4) the sign of possible α values is negative rather than positive.

3.5 Robertson's test theory

Robertson's [68] test theory is an early and important historical example of a test theory for relativity. Yet it was not the first. Eddington's test theory for relativistic gravity, which I examine in Section 3.6, was formulated as early as 1922. Nevertheless, Robertson's work is of special interest to philosophers because Robertson, more than Eddington or any other physicist today, articulates his concerns about the need for test-theory constructions in modern physics. In this section I will first present briefly Robertson's test theory, and then examine his motivation for its construction.

Robertson's derivation of his test theory's basic equation omits the Relativity Principle while retaining the Light Postulate. We can expect Robertson's test theory to be more complex than the test theory for relativistic kinematics presented in Section 3.4, because the Light Postulate bears only one part of the frame transformation equations (the coefficient to the boost velocity v) whereas the Relativity Principle is more governing of the overall form of these equations. Omitting the Relativity Principle in fact yields a test theory possessing three test-theory parameters.

As with the test theory in Section 3.4, the basic equation in Robertson's test theory is a transformation

$$x^i \rightarrow \xi^\mu = a_i{}^\mu x^i \tag{3.10}$$

between two inertial coordinate systems. At the beginning of Robertson's derivation of his basic equation, there are, of course, sixteen non-zero transformation coefficients $a_i{}^\mu$. Following Einstein, Robertson reduces this number to three independent coefficients by recourse to (1) certain innocuous background assumptions concerning the character of space and time, (2) the Light Postulate, and (3) by making appropriate coordinate choices, such as choosing the boost direction to lie along a coordinate axis. Unlike Einstein, however, Robertson does not apply the Relativity Principle anywhere in his derivation.

The result of this derivation can be expressed in matrix form:

$$(a_i{}^\mu) = \begin{pmatrix} a_0{}^0 & va_1{}^1/c & & \\ va_0{}^0/c & a_1{}^1 & & \\ & & a_2{}^2 & \\ & & & a_2{}^2 \end{pmatrix}. \tag{3.11}$$

Here, v is the boost velocity along the x^1 direction and c is the velocity of light.[24] The three velocity-dependent quantities $a_0{}^0(v)$, $a_1{}^1(v)$, $a_2{}^2(v)$ are the test-theory parameters to be empirically determined. Robertson does impose on these parameters the condition that each tend to unity as $v \rightarrow 0$, but this merely expresses his presupposition that relativistic kinematics will depend only on the boost velocity v, rather than on, for example, location

[24] Robertson has, incorrectly, $va_0{}^0$ instead of $va_0{}^0/c$ and $va_1{}^1/c^2$ instead of $va_1{}^1/c$.

or orientation. This presupposition is in fact built into Robertson's assumptions of spacetime homogeneity and spatial isotropy.

From the basic equation in his test theory, Equation 3.10, Robertson derives relations between observable quantities, which he subsequently uses empirically to fix his three test-theory parameters. He begins by assuming the metric in the coordinate system $\xi^{\mu}=(\tau, \xi, \eta, \zeta)$ to be the Minkowskian metric:

$$d\sigma^2 = d\tau^2 - (d\xi^2 + d\eta^2 + d\zeta^2)/c^2 \ . \tag{3.12}$$

He then uses Equation 3.11 to show that in the boosted system $x^i=(t, x, y, z)$ this metric becomes

$$d\sigma^2 = g_0^2 dt^2 - [g_1^2 dx^2 + g_2^2(dy^2 + dz^2)]/c^2 \ , \tag{3.13}$$

where

$$g_0 = (1 - v^2/c^2)^{\frac{1}{2}} a_0{}^0, \quad g_1 = (1 - v^2/c^2)^{\frac{1}{2}} a_1{}^1, \quad g_2 = a_2{}^2 \ . \tag{3.14}$$

From this result together with the equation $d\sigma = 0$ of the light cone Robertson shows that in the boosted coordinate system light will travel a distance l in either sense along a direction at angle h to the $x-$ axis in time

$$t = (l/cg_0)(g_1^2 \cos^2 h + g_2^2 \sin^2 h)^{\frac{1}{2}} \ . \tag{3.15}$$

He then points out that the results of the Michelson-Morley experiment support the proposition that "[t]he total time for light to traverse, in free space, a distance l and to return is independent of its direction". Therefore, we must have $g_1 = g_2$ and hence, from Equation 3.14, $a_2{}^2 = a_1{}^1(1 - v^2/c^2)^{1/2}$.

As a consequence, Equation 3.15 is reduced to $t = lg_1/cg_0$. Robertson then states that the results of the Kennedy-Thorndike experiment support the proposition that "the total time for light to traverse a closed path in coordinate system x is independent of the boost velocity v". This phenomenon, taken together with the fact that for $v = 0$ we must have $g_1/g_0 = 1$, implies that $g_0 = g_1$ for all v.

Using Equation 3.14 and the results just obtained, Robertson sets $g(v) \equiv g_0(v) = g_1(v) = g_2(v)$ and hence

$$a_0{}^0 = a_1{}^1 = g(v)/(1 - v^2/c^2)^{\frac{1}{2}}, \quad a_2{}^2 = g(v) \ . \tag{3.16}$$

Only $g(v)$ remains to be determined. Robertson first uses Equations 3.12 and 3.16 conjointly to derive a relativistic doppler shift formula, dependent on $g(v)$ and $(1-u^2/c^2)^{1/2}$. Then, he appeals to the results of the Ives-Stillwell experiment which support the proposition that "the frequency of a moving atomic source is altered only by the factor $(1 - u^2/c^2)^{1/2}$, where u is the velocity of the source with respect to the observer". This phenomenon clearly implies that $g(v) = 1$, from which it follows that Equation 3.13 becomes the Minkowski metric and Equation 3.10 becomes the Lorentz transformation.

Thus, Robertson has used his test theory to deduce from phenomena the Lorentz transformation, but without employing Einstein's Relativity Principle as a premise. This principle, which implies that the metric coefficients in Equation 3.13 are independent of the boost velocity v, Robertson in fact replaces with phenomena. (The title of Robertson's paper is "Postulate *versus* Observation in the Special Theory of Relativity".) In this way, Robertson has conditioned empirically an element of theory which constituted a premise in Einstein's argument for the Lorentz transformation. Robertson does not state explicitly that the particular element of theory parameterised by his test theory is the "rigging" structure of spacetime (which distinguishes a privileged rest frame). However, Mansouri and Sexl [52], who have modified Robertson's test theory, do make this assumption explicit. I will discuss the Mansouri-Sexl test theory further in Section 4.4.

Robertson introduces his test theory paper by discussing Einstein's seminal 1905 paper on relativity. He mentions explicitly the deductive character of Einstein's derivation of the Lorentz transformation from postulates. Robertson insists, however, that an even firmer empirical argument for the Lorentz transformation than Einstein's own is possible:

> Because of the revolutionary character of the postulates and consequences of this theory, there is discernible in the subsequent decades a certain reluctance whole-heartedly to accept its necessity, a reluctance shared at times even by scientists whose own work paved the way to, or confirmed the predictions of, the theory. It may therefore be appropriate on this occasion to review the present status of the theory, with special reference to the question of the degree to which postulate can now be replaced by observation in deriving the kinematics on which the theory is based. This re-examination, from a unified point of view closely allied to Einstein's original program, will emphasize the decisive nature of the two great optical experiments of Kennedy and Thorndike (1932) and Ives and Stilwell (1938) which have been performed in the interim, experiments which were designed and carried out for the explicit purpose of testing aspects of the Lorentz transformations which are insensitive to the Michelson-Morley experiment. We shall find, in confirmation of conclusions drawn by Kennedy and by Ives, that these three second-order experiments do in fact enable us to replace the greater part of Einstein's postulates with findings drawn inductively from observations.[25]

Evidently, the direct inspiration for Robertson's work is Einstein's own great achievement. More important, however, than the bare fact that Robertson's work concerns Einstein's theory is the deductive character of Robertson's reasoning. Robertson follows Einstein by deducing the basic equation of his test theory for relativistic kinematics from certain high-level innocuous assumptions (which, however, do not include the Relativity Principle). Then, Robertson uses his test theory to further deduce from phenomena the Lorentz transformation.

[25]Robertson [68], p. 378.

Puzzling, then, is Robertson's mention of induction at the very end of the introduction to his article. Robertson remarks that he is able to "replace the greater part of Einstein's postulates with findings drawn inductively from observations". However, Robertson's argument appears to be deductive through and through, so how should we interpret this remark? In my view Robertson's statement (and not only this statement but others of his on induction) indicates an alertness to a feature of scientific inquiry which Einstein does not make explicit in his work on relativity, and which thus serves to distinguish Einstein's work, methodologically, from Robertson's, and also, for that matter, from Newton's.

Because it involves a test theory explicitly, Robertson's work is already closer to Newton's than it is to Einstein's. In addition, it involves induction explicitly. Newton, recall, used his test theory to obtain the form of the inverse-square law. To discover the scope of this law, however, required inductions based on his test-theory results. Robertson appears to have the same thing in mind. In saying that it is possible to "replace" the Relativity Principle Robertson does not, I think, mean that just one or two phenomena can take the place of a principle as basic and as comprehensive as the Relativity Principle. What Robertson is asking is that we discover the true scope of this principle by inductions based on repeated and varied derivations from phenomena of the sort contained in his article. Although Robertson never mentions Newton, it is clear, I believe, that in referring to induction in this context he has something like Newton's Third Rule of Reasoning in mind.[26]

Standing in contrast to this explicitly "inductive" element in Robertson's (and Newton's) work, is Einstein's purely deductive approach from postulates assumed to be universally true. Indeed, Robertson almost apologises for the purely deductive character of Einstein's method when he writes

> At the time this [i.e. Einstein's] work was done an inductive approach could not have led unambiguously to the theory proposed [special relativity], for the principal relevant observations then available, notably the "ether-drift" experiment of Michelson and Morley (1886, could be accounted for in other, although less appealing, ways.[27]

Robertson appears to be claiming that in order to rule out these other "less appealing ways" (which presumably invoked an ether of some kind) further observations, such as those made later by Kennedy and Thorndike and by Ives and Stilwell, were required. According to Robertson, Einstein, in effect, made up for a lack of evidence by asserting the Relativity Postulate directly.

However, we know that Einstein had empirical evidence for the truth of this postulate—evidence which he presents explicitly at the beginning of

[26]In Section 4.6 I will argue that to reason cogently his way to curved spacetime Einstein must have (at least tacitly) employed methodological principles of a kind closely related to Newton's Rules of Reasoning—rules which, as we have seen, are involved in the inductive, rather than in the deductive, part of Newton's method of analysis.

[27]Robertson [68], p. 378.

his 1905 relativity paper. What Robertson demands over and above Einstein, it seems, is a rigorous formal connection between observations and that part of special relativity which is conditioned by the Relativity Postulate. Apparently, in 1949 Robertson was in a better position, empirically speaking, to establish such a connection, than was Einstein in 1905. Furthermore, Robertson's "inductive" approach leaves open the possibility that the Principle of Relativity might, in some domain, only be approximately true, whereas Einstein's 1905 assertion of the truth of this principle had dogmatic overtones that Einstein himself probably would rather have avoided.

We should not forget, of course, the important philosophical cum quasi-empirical motivation for Einstein's Relativity Principle, which I discussed in Section 3.3. This was Einstein's growing conviction, based on the fecundity of Maxwell's equations and the empirical problems facing the ether conception, that physical fields did not require a mechanical ether to support them. Indeed, Einstein's eventual rejection of the ether on this basis made possible the very idea of the Relativity Principle. Robertson, however, is silent on reasons Einstein might have had for advancing this principle, though quite clearly (and it seems mistakenly), he regards Einstein as having had little empirical warrant for this principle.

In this light, it is easy to see why Robertson sought to "replace" the Relativity Postulate with observations, rather than the easier task—undertaken later by other physicists—of replacing the Light Postulate in the same way. Robertson, I believe, was already convinced of the Light Postulate's robust empirical status, and thought it a greater imperative to bring under the check of experience the more abstract, and what seemed to him less well supported, Principle of Relativity.

Also noteworthy is Robertson's nonchalance towards the many "confirmed predictions" of special relativity theory. He does not appear to regard these confirmed predictions as supportive of the theory generally, or of the Relativity Postulate more particularly. Robertson, like Newton, demands a more stringent connection between theory and evidence, and it was to this end that he developed his test theory. Specifically, he sought a connection which would clarify the empirical basis of the Einstein's Relativity Postulate, that would test "aspects of the Lorentz transformations which are insensitive to the Michelson-Morley experiment". Evidently, Robertson was interested in the project of bringing particular pieces of evidence to bear on particular pieces of theory, a project which he regarded as relevant also to warranting relativistic gravity theory, as we shall see in the next section.

3.6 A test theory for relativistic gravity

Perhaps the earliest test theory after Newton's is the one for relativistic gravity devised by Arthur Eddington [20] in the early 1920s, and later elaborated by H. P. Robertson [69] in 1961 and L. I. Schiff [72] in 1967. This test theory is based on general relativity theory, and is in fact a natural relativistic successor to Newton's test theory, because it concerns primarily centripetal-type motions due to gravity in the Solar System.

The basic equation in Eddington's test theory is a generalised spacetime metric representing the gravitational field about a stationary, non-rotating, spherically symmetric body of mass M:

$$\begin{aligned} ds^2 &= -\left(1+\frac{a_1}{r}+\frac{a_2}{r^2}+\dots\right)^{-1} dr^2 - r^2(d\theta^2+\sin^2\theta d\phi^2) \\ &\quad + \left(1+\frac{b_1}{r}+\frac{b_2}{r^2}+\frac{b_3}{r^3}+\dots\right) dt^2 . \end{aligned} \tag{3.17}$$

From this metric Eddington derives various relations between observable quantities which allow him empirically to determine some of the free parameters appearing.[28] For example, phenomena supporting Newton's inverse-square law give $b_1 = -2M$, light-bending by the sun gives $a_1 = -2M$, while the motion of Mercury's perihelion gives $b_2 = 0$. According to Eddington a_2 and b_3 were not able to be determined with any high accuracy, and there were no phenomena then known which could be used to determine the values of higher-order parameters. The parameter values which could be determined, however, agreed with the values dictated by general relativity theory.

Eddington [20] motivates his test theory by asking whether experiments actually compel our adoption of the physical laws that we adopt. In his book "The Mathematical Theory of Relativity" Eddington considers this question in relation to the logical structure of physical theory, a discussion of which

> ... would be divided into two parts, the one showing the gradual ascent from experimental evidence to the finally adopted specification of the structure of the world, the other starting with the specification and deducing all observational phenomena. The latter part is especially attractive to the mathematician for the proof may be made rigorous; whereas at each stage in the ascent some new inference or generalisation is introduced which, however plausible, can scarcely be considered incontrovertible. We can show that a certain structure will explain all the phenomena; we cannot show that nothing else will.

Eddington's two-part logical analysis of physical theory seems quite similar to Newton's division of experimental philosophy into the methods of analysis and composition. Without speculating further, however, it is evident from the rest of Eddington's discussion that he, like Robertson, clearly harbours an epistemic concern: how can inferences from phenomena to theory be made as rigorous as possible? Eddington's answer, like Robertson's, is to construct a test theory by which phenomena can be employed to constrain elements of theory.

Eddington believes, therefore, that experiments can compel us to adopt the physical laws that we adopt, though only "within certain limitations".

[28] Eddington [20], p. 105ff.

It is clear from Eddington's discussion that his phrase 'within certain limitations' indicates a sensitivity to both the inductive aspects of theory confirmation and to the necessity of positing theoretical background assumptions. I have indicated in my various historical case studies so far the necessity of background assumptions if the test-theory method is to be at all possible. I have also clarified the role of induction in relation to this method. Thus, I accept Eddington's phrase " within certain limitations", though I do not think that the limitations in question diminish the compelling nature of test-theory confirmation. In Chapters 4 and 5 I discuss further what Eddington refers to as the limitations of inferring theory from experiment.

Robertson's contribution to Eddington's programme was primarily to draw attention to Eddington's test theory in light of recent possibilities opened up by space age astronomy. Robertson believed that our ability to place satellites and other observing facilities in space would make possible a more accurate determination of the parameters appearing in this test theory.

Schiff developed Robertson's suggestions further, and derived from Eddington's test-theory metric expressions for the precession of an orbiting gyroscope, the second-order gravitational red shift, and the transit time of radar signals reflected from Mercury or Venus. Because the expressions Schiff derives contain the same parameters as other expressions relevant to the three "classical tests of general relativity", Schiff's work shows how some of Eddington's test-theory parameters may be overdetermined by phenomena. For example, the phenomena of light deflection by the Sun and the transit time of radar signals to inner planets allow for an overdetermination of the parameter which quantifies the extent to which matter curves three-dimensional space. Schiff calls Eddington's approach, which he imitates, a "quantitative method" and later a "test method", which is perhaps the origin of the term 'test theory', first used, apparently, by Reza Mansouri and Roman Sexl [52].

Both Robertson and Schiff indicate in their discussions how Eddington's test theory allows them to keep track of exactly what part of the relativistic theory of gravity is being determined, or tested, by a particular phenomenon. It thereby allows them to concentrate their efforts, and the effort of experimentalists, on parts of relativity theory which remain weakly supported, or even untested, by phenomena. In Section 3.8 I will examine one such part, the so-called gravitomagnetic part, and I will criticise Ken Nordvedt's test theory argument to the conclusion that this element of theory has already been measured to quite high accuracy.

Eddington's test theory is pioneering, but crude, for it applies only to very simple physical systems. In Eddington's day, and even in Robertson's and Schiff's, this was not a cause for worry, for technology at that time was not sufficiently discriminating to detect relativistic deviations from the ideal assumed by Eddington. In contrast, modern, highly sophisticated measuring devices, positioned both on Earth and in space, have, as Robertson and Schiff predicted, opened up vast possibilities for the testing of previously untested elements of general relativity theory. The excitement generated in this field, together with the discovery of pulsars, quasars, and the cosmological background radiation, as well as important theoretical results, has also led to the development of a plethora of alternative gravity theories to general relativity theory. The Parametrized Post-Newtonian Formalism, which is based on Eddington's work, and which I examine in the next two sections, is a test theory for relativistic gravity which is sufficiently complex to allow both for the empirical determination of many previously untested elements of relativistic gravity theory, and for the representation of many rival theories to general relativity.

3.7 The Parametrized Post-Newtonian (PPN) Formalism

The Parametrized Post-Newtonian (PPN) Formalism is a generalisation of Eddington's test theory, which I discussed in the previous section. There are in fact two ways of generalising Eddington's test theory. These are: (1) to include in Eddington's metric higher order terms (the existence of such terms is simply a consequence of the non-linearity of relativistic gravitation), and (2) to modify the defining assumptions of Eddington's test theory to allow for gravitational sources more complex than Eddington's single, stationary, non-rotating, spherically symmetric body. One can of course combine procedures (1) and (2) for even more generality, but the complexity of the resulting test theory increases rapidly as one goes to higher orders and includes less idealised sources.

Procedure (2) has been carried out in part by the physicists Ken Nordvedt and Clifford Will.[29] The result, which incorporates matter described by a perfect fluid, is what today is called the parametrized Post-Newtonian Formalism. This test theory for relativistic gravity has been extended even further to include point masses bearing electric charge, fluids with anisotropic stresses, bodies with strong internal gravity, and post-post-Newtonian effects.[30] The last-mentioned extension carries out procedure (1) on the PPN Formalism to the next order. To date, however, no post-post-Newtonian effects have been observed, because of their extreme smallness relative to Newtonian and to post-Newtonian effects. In principle, though, there is nothing barring procedure (1) from being carried out to any order.

According to Nordvedt and Will, their aim in developing the PPN framework was to "systematise the comparison between theory and experiment".[31] The basic equation of this test theory is the so-called PPN metric:

[29]Nordvedt [58], Will [85], Will and Nordvedt [86].

[30]For references containing these extensions of the PPN Formalism see Chapter 4 of Will [87].

[31]Will and Nordvedt [86], p. 757.

$$
\begin{aligned}
g_{00} &= -1 + 2U - 2\beta U^2 + (2\gamma + 2 + \alpha_3 + \zeta_1)\Phi_1 - \zeta_1 \mathcal{A} \\
&\quad + 2(3\gamma + 1 - 2\beta + \zeta_2)\Phi_2 + 2(1 + \zeta_3)\Phi_3 + 2(3\gamma + 3\zeta_4)\ \Phi_4 \\
&\quad - (\alpha_1 - \alpha_2 - \alpha_3)w^2 U - \alpha_2 w^{ij} U_{ij} + (2\alpha_3 - \alpha_1) w^i V_i\ , \\
g_{0j} &= -\tfrac{1}{2}(4\gamma + 3 + \alpha_1 - \alpha_2 + \zeta_1)V_j - \tfrac{1}{2}(1 + \alpha_2 - \zeta_1)W_j \\
&\quad + \tfrac{1}{2}(\alpha_1 - 2\alpha_2) w_j U + \alpha_2 w^i U_{ij}\ , \\
g_{jk} &= (1 + 2\gamma U)\delta_{jk}\ . \qquad (3.18)
\end{aligned}
$$

There is in fact some flexibility in this test theory as to what test-theory parameters appear.[32] The nine PPN parameters appearing in Equation 3.18 $(\beta, \gamma, \alpha_1, \alpha_2, \alpha_3, \zeta_1, \zeta_2, \zeta_3, \zeta_4)$ have been used in linear combination in such a way as to allow a fairly unambiguous physical interpretation of each of them (which I shall provide in a moment). This set of parameters yields ten equations in nine unknowns and therefore yields a unique set of values for the PPN parameters.

The potential function U is just the Newtonian potential

$$
U(t, \mathbf{x}) = \int \frac{\rho(t, \mathbf{x}')}{|\mathbf{x} - \mathbf{x}'|} \qquad (3.19)
$$

of an extended matter distribution with Newtonian mass density ρ. The other potential functions in Equation 3.18 $(\mathcal{A}, U_{ij}, V_i, W_i, \Phi_1, \Phi_2, \Phi_3, \Phi_4)$ are similar in form to Equation 3.19.[33] These potentials contain, in addition to the rest density ρ, variables quantifying energy, pressure and velocity. For a system of point masses we may simply set $\rho = \sum_i M_i \delta(\mathbf{x} - \mathbf{z}_i)$, and make corresponding adjustments to the potential functions, as appropriate.

Evidently, the parameter γ is a measure of the curvature of space, while β quantifies the strength of the first-order non-linear deviation from the linear Newtonian contribution of $2U$ to the curvature of time. Both parameters take the value 1 in general relativity theory. The parameters $(\alpha_1, \alpha_2, \alpha_3)$ quantify preferred frame effects, while $(\zeta_1, \zeta_2, \zeta_3, \zeta_4)$ are measures of total momentum conservation violation. These parameters are all zero in general relativity theory, of course, though they are non-zero in some rival gravity theories.

Will is careful to point out that generally these interpretations of the PPN parameters "are valid only in the PPN gauge, and should not be construed as covariant statements", though γ can be so construed, as a calculation of the spatial components of the Riemann curvature tensor shows.[34] What Will calls the PPN gauge (in which Equation 3.18 is expressed) is a coordinate system attached to a frame which moves relative to the comoving

[32] Will and Nordvedt [86], p. 765. Note the signature change, which is (-1, 1, 1, 1) here, but (1, -1, -1, -1) in Will and Nordvedt's original paper, and also in Eddington's metric.

[33] Expressions for these potentials can be found on p. 95 of Will's book *Theory and experiment in gravitational physics* [87], which contains a thorough discussion of the PPN Formalism and also treats other modern test theories for relativistic gravity.

[34] Will [87], p. 115.

cosmological frame (as the Solar System moves relative to the expansion of the Universe). This coordinate system is quasi-Cartesian in the sense that the PPN metric approaches the Minkowski metric at large distances from the origin.

The conceptual presuppositions of the PPN Formalism include: (1) that the correct theory of gravity is a metric theory, which in most cases (though not all) implies that gravity is a geometrical phenomenon arising from the curving of four-dimensional spacetime by matter.[35] (I will take a closer look at this presupposition in Section 4.6.); (2) the postulates of differential geometry, which tell us how to make derivations from the PPN metric.

The defining assumptions of the PPN Formalism include: (1) that the source of gravity is a perfect fluid, which includes systems consisting of only a few widely separated particles, such as the Solar System; (2) that the magnitude of gravitational fields produced by the cosmological distribution of matter, and by bodies in the neighbourhood of the Solar System, are negligible. Will estimates, for instance, that cosmological effects will be smaller than the highest order effects generated by the PPN metric by a factor of 10^6. [36]

Will also points out that terms appearing in the PPN metric have been restricted to those which actually appear in already articulated theories of relativistic gravity. Terms containing $\Phi_1\Phi_3U^{-2}$ and $U_{ij}U_{ij}$, for example, do not appear, even though they are compatible with the PPN Formalism's defining assumptions.

These defining assumptions, taken together, are lax enough to allow a considerable variety of alternative theories of relativistic gravity to be represented. Whereas Eddington's test theory was based on general relativity, and merely parameterised possible deviations from it, the PPN Formalism represents already articulated alternative gravity theories which correspond to those deviations. This is possible, Will points out, because the post-Newtonian limits of metric theories of gravity "have a nearly universal form, except for the values of the PPN parameters".[37]

An important application of the PPN Formalism, beyond its primary function of determining empirically the elements of theory represented by the PPN parameters, is that of comparing and classifying alternative gravity theories. Will [87] undertakes such an analysis in Chapter 5 of his book, where he describes and compares many of the important contemporary rivals to general relativity theory. Will's analysis shows that with the help of the PPN Formalism one can classify alternative theories of gravity broadly into two groups—purely dynamical theories (like general relativity), and theo-

[35] In Section 4.6 I discuss briefly metric theories of gravity in which spacetime is flat and non-dynamical. Also, theories exist which describe gravity in terms of curved four-dimensional spacetime, but which are non-metric. For example, Cartan's curved spacetime formulation of Newtonian gravity theory.

[36] Will [87], pp. 91–92.

[37] Will [87], p. 116.

ries which have "prior geometry"—though Will draws still finer distinctions within these two groups.

According to Will, application of the PPN Formalism to various phenomena in the Solar System confirms that general relativity remains a viable theory of relativistic gravity, meaning that the theory's associated PPN parameter values lie within the tight bounds placed on these parameters by observation.[38] It is true that many other theories also remain viable, even though a large number of competitors have been ruled out by the test-theory method. However, the existence of rival theories does not diminish the fact that many elements of general relativity theory, which it shares with its rivals, have in fact been empirically confirmed. However, to ensure general relativity's empirical superiority over its still viable rivals it is Will's view that further careful observations of celestial systems beyond the Solar System are required.

3.8 Searching for gravitomagnetism with the PPN Formalism

An important feature of test theories is that they enable empirical evidence to condition specific parts of physical theory. In the present section I illustrate this feature of test theories by discussing how physicists will employ the PPN Formalism to condition a specific part of relativistic gravity theory, namely, the gravitomagnetic part. Usually, the parametric structure of test theories allows us readily to identify exactly which part of a physical theory a given phenomenon conditions. However, in some test theories, like the PPN Formalism, where there is flexibility in choosing test-theory parameters, interpretive problems can arise. In this regard I criticise a claim made by Ken Nordvedt, who has used the PPN Formalism to argue that the gravitomagnetic part of theory has already been empirically conditioned. In criticising Nordvedt's claim I highlight some subtleties of using test theories to measure specific elements of physical theory.

Gravitomagnetism is as essential to relativistic gravity theory as magnetism is to electromagnetic theory. Whereas magnetic effects arise from electrical currents, gravitomagnetic effects arise from gravitational currents, i.e. from momentum (both linear and angular). According to relativistic theory, the gravitational field of a spherically symmetric, non-rotating body will be different from the field produced by the body after it is set spinning. According to Newtonian physics, however, the gravitational field will remain the same, because Newtonian gravitational fields are generated by mass alone.

In the PPN metric (Equation 3.18) the gravitomagnetic part of relativistic gravity theory is represented by the "off-diagonal" terms g_{0j}. The potential functions V_j and W_j appearing in these terms arise, respectively, from the linear and angular momentum densities of the gravitational source.

[38] Will [88].

(No "off-diagonal" terms appear in Eddington's test-theory metric, because Eddington's test theory is restricted to stationary, non-rotating sources.) The g_{0j} terms give rise to gravitomagnetic effects. For example, the Earth's rotation, should, according to relativistic gravity theory, induce precession in an orbiting gyroscope. For a nearly circular, polar orbit the rate $\mathbf{\Omega}$ of gravitomagnetic precession is given by:

$$\mathbf{\Omega} = (\gamma + 1 + \tfrac{1}{4}\alpha_1)\frac{\mathbf{J}}{r^3} , \qquad (3.20)$$

where r is the radius of the orbit and $\mathbf{J}$ is the angular momentum of the Earth.[39] Once geodetic precession (which is a relativistic, but non-gravitomagnetic effect) has been subtracted, physicists can use the observed precession of the Stanford Gyroscope [71, 26] to determine the value of the PPN "coefficient" $(\gamma + 1 + \frac{1}{4}\alpha_1)$.[40]

It is widely held in the physics literature that, owing to their extreme smallness, gravitomagnetic effects have never been observed, and that the Stanford Gyroscope will provide the first evidence of their existence. However, Ken Nordvedt [59] has challenged this orthodox view. In one part of his critique Nordvedt uses the PPN Formalism to argue that the gravitomagnetic interaction "has been measured to 1 part in 1000".

Nordvedt [59] points out that for a spinning gravitational source, like the Earth, the components g_{0j} contribute a magnetic moment-like vector potential

$$\mathbf{h} = (\gamma + 1 + \tfrac{1}{4}\alpha_1)\frac{\mathbf{J} \times \mathbf{r}}{r^3} , \qquad (3.21)$$

which gives rise to the gravitomagnetic effects sought in various experiments, including the effect represented by Equation 3.20. Nordvedt then remarks that

> ...we already know much about the coefficients γ and α_1 which calibrate this vector potential. γ is known to within 1 part in 1000 by the latest radar time-of-flight experiments to Mars planetary landers (Hellings 1984). α_1 has been constrained by many experiments, but the most recent and tightest bound has been made by using binary pulsar system PSR 1913+16 observational data, which include excellent agreement between general relativity's prediction of gravitational radiation reaction effects and the measured orbital period secular rate of change; this agreement requires α_1 to be less than 10^{-6} (Nordvedt, 1987).[41]

From these results and Equation 3.21 Nordvedt concludes that

[39] Equation 3.20 has been derived from the PPN metric, taking into account the circular and polar character of the orbit. In this case, the assumptions of circularity and polarity can be regarded as auxiliary assumptions of the PPN Formalism. Will [87] states Equation 3.20 in a slightly more general form on p. 213 of his book. His expression applies to eccentric as well as to circular-type orbits.

[40] For a discussion of geodetic precession see pp. 208–210 of Will's book.

[41] Nordvedt [59], p. 1400.

> There exists a variety of accurate observations of various post-Newtonian gravitational effects which together measure the gravitational vector potential.[42]

The problem with this particular argument of Nordvedt's is it relies on phenomena which are not gravitomagnetic in origin. These phenomena arise from the mass, not from the momentum, of gravitating bodies. For example, the radar propagation times which Nordvedt cites determine the coefficient γ of purely mass-dependent terms in g_{ij}, not momentum-dependent terms in g_{0j}. Thus, I reject Nordvedt's test-theory argument for the existence of gravitomagnetism.

The origin of Nordvedt's misinterpretation can, I believe, be traced to his (and Will's) choice of PPN parameters. In Equation 3.18 some of the parameters (including γ) which appear in front of the gravitomagnetic potentials V_j and W_j appear also in front of other, non-gravitomagnetic potentials. Thus, in Nordvedt and Will's expression of the PPN metric there are no test-theory parameters associated uniquely with the gravitomagnetic part of the metric. Hence, it is not clear what phenomena must be invoked empirically to condition this gravitomagnetic part.

A choice of PPN parameters more suited to the empirical study of gravitomagnetism has been given by Misner, Thorne and Wheeler (MTW) [54]. MTW replace the strings of PPN parameters appearing in front of the potential functions V and W in Equation 3.18 with the much simpler coefficients $7\Delta_1$ and Δ_2, respectively.[43] This results in a gravitational vector potential of the form

$$\mathbf{h} = -(\tfrac{7}{4}\Delta_1 + \tfrac{1}{4}\Delta_2)\frac{\mathbf{J}\times\mathbf{r}}{r^3}, \tag{3.22}$$

and a corresponding equation for the gyroscope precession $\mathbf{\Omega}$, which also contains the coefficients Δ_1 and Δ_2. Because Δ_1 and Δ_2 appear only in the gravitomagnetic part of the PPN metric, there is no ambiguity about their gravitomagnetic origin. MTW's choice of PPN parameters shows clearly that to measure the gravitomagnetic part of the metric one must determine empirically the parameters Δ_1 and Δ_2, either separately or in combination. Nordvedt, however, nowhere makes any such determination.

Nordvedt's error reveals the importance of very carefully choosing test-theory parameters for the task of deciding which particular piece of theory a given phenomenon is relevant to and thus is able empirically to condition. This is important, at least, in test theories like the PPN Formalism, where parameters can occur at more than one place in the test theory's basic equation. In simpler test theories, like Newton's, where parameters occur only once, no such interpretive problems can arise.

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[42] Nordvedt [59], p. 1396.

[43] MTW interpret Δ_1 and Δ_2 as quantifying the amount of "frame dragging" produced by the angular momentum of the source. For a discussion of frame dragging see Gunn [33], Section 6.2.

While it is clear that the Stanford Gyroscope experiment will allow for an unambiguous determination of the purely gravitomagnetic part of relativistic gravity theory, it is also clear that the results of this experiment will not allow us to determine separately the parameters Δ_1 and Δ_2. The gravitomagnetic part of relativistic gravity is itself composed of two parts, one due to the linear the other due to the angular momentum of the gravitating source, and the gyroscope precession, which is sourced by both types of momentum, will only measure the two parts in combination. In other words, a one-to-one relationship between the phenomenon and a single test theory parameter does not exist in this case, and there will be many more cases like this one within the PPN formalism. Indeed, this feature will be endemic to many-parameter test theories, and indicates, it must be admitted, a limit to the extent to which phenomena may be put into relation with very specific parts of theory.[44] This limit does not arise from any deficiency of test theory methodology, but from the simple fact that many phenomena are "composite" in character—they owe their origin to sources described by several parts of the theory, not just one specific part. There is no concession here to the holist belief that only entire theories (or the whole of science) may be tested by phenomena. For it remains the case that some phenomena will be able to condition a small number of theoretical elements in combination (as in the gravitomagnetic example above), and some will even be able condition individual elements. Holism denies these possibilities.

3.9 Modern test theories and the future of physics

There is one rather striking difference between Newton's application of his test theory and the application of test theories by physicists today. Newton, as we have seen, used his test theory to discover new physical theory, whereas modern test theories seem to have been developed primarily to confirm already existing, and already widely accepted, physical theory. Newton plainly regarded his methodology as having a progressive function, whereas modern physicists seem to regard test theories as fulfilling a more conservative role. One aim of this dissertation is to alert physicists today to Newton's demonstration that test theories can be used for more than simply testing physical theories. The example of Newton shows physicists in fact how test theories can be used creatively to develop new, and more unifying, physical theories of greater explanatory power.

Nevertheless, there are features of modern test theories (that in fact all test theories share) which may already be construed in a progressive,

[44] I owe this important observation to Geoff Stedman. Stedman himself has proposed a way of assessing this limit in specific cases. His idea is to set out a tree-like hierachy of test theories, in which each daughter test theory has one more parameter than its parent, and allow phenomena to discriminate between the test theories in the tree. On my understanding of Stedman's view a parameter will be "essential for physics" just in case it can be empirically determined independently of the other parameters.

rather than in merely a conservative, way. For a start, modern test theories systematically represent within their structures whole classes of alternative physical theories. There is no guarantee that the physical theory (if there was just one theory) that provided a basis for the original development of some test theory will continue to remain viable by the lights of that test theory. It may occur, for example, that application of the PPN Formalism eventually rules out general relativity as a viable theory of relativistic gravity in favour of other theories represented by the formalism. In this case a genuine theoretical discovery would have occurred, and the PPN Formalism would have performed a genuinely progressive function.

Unification of the fundamental physical interactions is an important goal of modern theoretical physics. Since gravity is one of the fundamental interactions, current gravity theory will play a crucial role in this unification process. Test theories for relativistic gravity, such as the PPN Formalism, enable phenomena very strongly to confirm physical theory, as I have shown in the present chapter. Hence, these test theories ensure that the theoretical basis for further physical theorising is itself empirically robust. They establish, that is, the conditions necessary for the further unification of physics. In this way modern test theories are already performing, if indirectly, a progressive function. I emphasis again, however, that I wish eventually to argue (in Section 6.2) that test theories are able not only to establish the conditions necessary for unification, but that they may also be instrumental in forging that unification.

3.10 Conclusions

In this Chapter I have presented some modern test theories and I have described their character and use. In my view the existence and fruitful application of test theories for relativity indicates that Einstein's spacetime theories are not only profoundly unifying of experience, but are also strongly conditioned by experience, and are thus preeminent examples of scientific knowledge.

Modern test theories show explicitly how apparently disparate phenomena are brought into connection with one another by relativity theory, and thereby unified. Modern test theories do this by showing how different phenomena may condition empirically the same element of theory. For example, the bending of light by the Sun and the "anomalous" delay in radar propagation times both determine the PPN parameter γ to be unity within a very small margin. Thus, test-theory analysis can indicate what particular element of theory gives rise to both these phenomena. In the PPN case, the element of theory in question is the curvature of three-dimensional space. Hence, an advantage of test-theory analysis is that in certain cases (though hardly all) it is able accurately to pinpoint the unifying theoretical element, or elements, underlying phenomena which may otherwise appear to be quite unrelated. This function, test theories for relativity have performed for a wide variety of celestial phenomena.

Evidently, test theories also show that relativity theory is strongly conditioned by experience. If two apparently disparate phenomena determine a given theoretical parameter to be the same value, then they overdetermine the value of that parameter. Via test theories, many elements of Einstein's relativity theories have been richly overdetermined by phenomena. It is true that these empirical determinations do not always single out Einstein's theories from their rivals. However, this is simply because the empirically conditioned elements of theory are shared by Einstein's theories and their rivals. It does not follow that Einstein's theories have been confirmed any less.

In fact, test theories allow one the luxury of a kind of agnosticism about theories. One can remain committed to those elements of theory which have been richly overdetermined by the evidence, yet not be tied to any particular physical theory. In this way test theories may provide physicists with a solid theoretical foundation from which to initiate theory change, as Einstein did when he used only one part of classical electromagnetic theory (constancy of light speed) to derive special relativity theory, but remained uncommitted to the theory as a whole because of its shaky relation to black body phenomena. Indeed, this feature indicates a further way, in addition to those mentioned in Section 3.9, in which test theories may depart from their merely conservative role in theory confirmation.

By now it should be clear that modern test theories are constructions of exactly the same sort as Newton used to argue for the inverse-square character of forces acting in the Solar System. Both Newton and modern physicists have set up methodological frameworks for empirically selecting physical theories from out of a class of parameterised alternatives, and they have used these frameworks empirically to determine, and often to overdetermine, many elements of high-level physical theory. Thus, modern test-theory constructions have helped physicists to give to Einstein's spacetime theories what Newton called "the highest evidence a proposition can have in [experimental] philosophy".[45]

Furthermore, the appearance of test theories in influential texts, like Misner, Thorne and Wheeler's "Gravitation" and Will's "Theory and experiment in gravitational physics" shows that an important element of Newtonian method has not only passed into the reflexive practice of modern physicists, but now forms part of its conscious method. It is part of how present day physicists are taught to work.

As a result, the practice of modern physicists seems closer to Newton's practice than to the practice of Einstein, who did not, it appears, devise or use any test theories. However, it would be a mistake to underestimate the influence of Einstein on current methodology, and in this regard the following three points should be remembered. (1) Developers of test theories never mention Newton in connection with test theories. Their concern is with Ein-

[45] See my quotation of Newton on page 28.

stein's physics, and their goal is to come to terms with his innovations. In doing this, they have been very much influenced by Einstein's own approach to spacetime theories. (2) In particular, the rigorous deductive character of Einstein's arguments from postulates has inspired numerous similar arguments. Test theories exemplify this pattern. Their construction typically follows Einstein's deductive model, and their employment actually takes it further by "replacing" some of Einstein's postulates with deductions from phenomena. (3) Of profound importance, in my view, to the possibility of Einstein's relativity arguments, and to the possibility of modern test theories generally, was Einstein's radical "Newtonian" attitude towards field theory. Einstein saw his way past the temptation to provide an intelligible mechanical reduction of fields. This weakened, I believe, his commitment to the problematic ether, which he eventually abandoned, and paved the way to the Relativity Principle. Relegating the rationalist demand for intelligibility below the demand for a strong connection between theory and evidence also made it easier, I believe, for Einstein to accept the radical conceptual revisions demanded by his relativity arguments.

Einstein's "Newtonian" attitude is clearly reflected in the work of later physicists, especially that of H. P. Robertson. Robertson's test theory was motivated by a concern that some scientists found the "revolutionary" (i.e. counter intuitive) character of Einstein's theories difficult to accept. Robertson's response was not to try and make Einstein's ideas more intelligible, but simply to strengthen further the relationship between these ideas and the evidence.

It is a fortunate thing for our understanding of spacetime physics that modern test-theory constructions so closely resemble Newtonian methods. For, it allows us to conclude that despite their traditionally having been taken to have a metaphysical status, spacetime principles can in fact be put into strong connection with phenomena, in precisely the same way that Newton put his force law of gravity into strong connection with phenomena.

CHAPTER 4

Spacetime, causation and limits of test-theory methodology

Newton used his test theory to discover empirically a deep element of physical theory, the inverse-square law of gravity. Modern test theories for relativity reach even deeper, to the very spacetime principles which dictate the general form of physical laws such as Newton's. Thus, modern test theories enable physicists today empirically to condition elements of physical theory which previously constituted the conceptual presuppositions of Newton's test theory. This successful extension of test-theory methodology to spacetime principles demonstrates the power of the test-theory method. But are there limits to that power? How deep can the test-theory method reach? Is there some level of physical theory at which this method will cease to apply? In asking these questions I am asking about the furthest limits of test-theory application. I seek to answer by examining, in the present chapter, the relevance of test-theory methodology to specific conceptual issues in the foundations of physics. I believe that a study of these issues will enable us to understand more clearly both the limits of test-theory methodology and the extent to which the conceptual foundations of physics are empirically determinable.

The foundational issues I discuss in this chapter concern: (1) the status of distant simultaneity in relativistic kinematics, (2) the status of spacetime geometry in relativistic gravity theory, (3) the question of efficient versus final causation in mechanics, and (4) the question of causal indeterminacy in quantum physics. This last issue I discuss only briefly, and regard it as an important line of inquiry for future research, though I consider in passing whether the test theory method could, and indeed should, be applied in the practice of quantum physics. With regard to issues (1) and (3) I show that test theories today cannot secure for us determinate simultaneity relations, nor can they help us to choose between efficient and final cause conceptions in mechanics. These issues, I conclude, lie beyond the current reach of the test-theory method. Issue (2), however, lies right at the farthest reach of this method. Test theories for geometrical relations are certainly possible in relativistic gravity theory, on the assumption that gravity is metrical in character. This assumption itself, however, cannot be warranted by test theories alone, but only once less formal methodological principles are also brought to bear.

These results, and general historical facts about the conceptual development of physics, make room in physics, I contend, for metaphysical argument and conceptual analysis. I argue, moreover, that test-theory methodology can help us to distinguish the plainly empirical from the more metaphysical, or philosophical, aspects of practice in physics. I conclude, nevertheless, that the inability of current test theories to condition certain highly constitutive elements of physical theory provides no guarantee that these elements will be forever immune from such empirical conditioning.

In Section 4.1 I discuss, in a general way, the nature and scope of test-theory methodology. In Section 4.2 I relate my discussion of the method's scope to the problem of interpreting physical theory. In Section 4.3 I introduce the ever controversial topic of distant simultaneity in relativistic kinematics. In Section 4.4 I discuss attempts to run test-theory arguments for distant simultaneity. In Section 4.5 I examine some arguments for curved spacetime advanced recently by philosophers of science. In Section 4.6 I present a test-theory-based argument for the Principle of Equivalence, and I evaluate the claim that this argument is also an argument for spacetime being curved. In Section 4.7 I discuss test-theory methodology in relation to efficient and final cause formulations of mechanics. In Section 4.8 I consider the possibility of test theories for that branch of physics which appears to have radical implications for our understanding of causation, namely, quantum mechanics. Finally, in Section 4.9 I draw some conclusions about the limits of test-theory methodology, and about the role of philosophical inquiry in physics.

4.1 The nature and scope of test-theory methodology

Imagine that our beliefs about the physical world are spread out along a continuum. Towards one end of this continuum—the observational end—are located beliefs about very specific phenomena, beliefs which are relevant, say, to just a few material bodies. At the other end of this continuum—the theoretical end—are located very general beliefs about properties common to most or all bodies. Now the test theories I have examined in Chapters 2 and 3 condition beliefs which lie perhaps halfway between the middle and the theoretical end of the continuum. However, the actual construction and application of these test theories draw on beliefs which lie closer to both ends. For example, Newton's test theory concerns the determination of specific forces acting in the Solar System, yet this test theory was made possible by an abstract dynamical theory (comprising Newton's Laws of Motion), and its application required both abstract mathematical beliefs about the geometry of space as well as more phenomenal beliefs about the behaviour of bodies in the Solar System. Thus, test theories tend to draw together beliefs from both ends of the continuum and focus them on beliefs at intermediate locations.

By inquiring after the limits, or scope, of test-theory methodology I am asking how close to either end of the belief continuum we can direct the focus of our test theories. The fundamental constituents of test theories, namely, their basic equations, express mathematical relationships (albeit somewhat indeterminate ones) between physical quantities. So, the observational limit of test-theory methodology would seem to be the point where beliefs about the physical world become so specific or qualitative that they no longer concern mathematical relationships. There are in fact a multitude of such beliefs. What is also clear, however, is that test theories for laws of a less highly theoretical, and more phenomenological, sort are clearly possible. I present one such test theory explicitly in Section 4.6. Indeed, the kind of empirical fixing of theoretical parameters that is such a crucial part of test-theory methodology, is in fact a routine part of the practice of experimental physics. Test-theory methodology shows that what is routinely practised at low levels of inquiry in physics is also possible at much higher levels.

However, the upper or theoretical limit of the test-theory method is, I believe, less easy to discern than its observational limit. For a start, the most fundamental of physical beliefs typically express relationships between mathematical quantities, so we cannot invoke the mathematical character of test theories to define for us their theoretical limit, as we could invoke it to define their observational limit. Of course the method is a quantitative one, so it will obviously not reach to beliefs about numbers, which all test theories presuppose. By analogy we might think that even the more specific of the conceptual presuppositions of our test theories are beyond the ken of test-theory methodology. However, the example of Newton steers us away from drawing this conclusion. Newton's test theory presupposed beliefs about the geometry of space, but in modern times physicists have constructed test theories which enable them empirically to condition such beliefs. Rather than trying at this stage to give a general account of the upper or theoretical limit of test-theory methodology, I will study this limit piecemeal by way of discussing specific conceptual problems in the foundations of physics.

4.2 Interpreting physical theory

The issue of whether we can direct the focus of our test theories onto certain fundamental physical beliefs obviously bears on the question of whether those beliefs are empirically conditionable. This issue also bears, however, on the interpretation of other, less fundamental beliefs which are, without question, empirically conditionable. The reason is that physical beliefs at the theoretical end of our belief continuum themselves theoretically condition beliefs which lie closer to the observational end. That the PPN Formalism enables us to determine empirically the geometry of three-dimensional space depends, for example, on the more fundamental proposition that the geometry of space is affected in some way by matter. Similarly, that Newton's test theory allowed him to determine the distance dependence of celestial forces depended on the more fundamental proposition that material bodies interact with one another in an efficient causal way by means of impressed forces.

But what if we could alter the conceptual presuppositions of our test theories and yet retain their ability empirically to measure theoretical parameters? What if, for example, we could abandon the PPN Formalism's presupposition that the gravitational field is a metric field and yet still be able to determine the test-theory parameter γ associated formerly with the curvature of space? What if we could reformulate Newtonian mechanics using final cause conceptions, and yet still be able to determine the test-theory parameter n associated formerly with the distance dependence of forces?

Given such possibilities, and given that these alternative conceptual frameworks really say something different from one another, then exactly what our test theories could determine would seem to be ambiguous. In such cases our interpretation, even of empirically overdetermined elements of physical theory, would appear to be underdetermined. By studying in this chapter the relevance of test-theory methodology to various conceptual issues in the foundations of physics, I aim to see whether, and to what extent, this potential problem of interpretive underdetermination might be solved in specific cases.

4.3 Distant simultaneity in relativistic kinematics.

Debate among physicists and philosophers over the objective status of distant simultaneity relations has a long history, and continues still. In recent times some physicists have used test theories to argue that distant simultaneity relations are empirically determinable and therefore objective. Other physicists have criticised their arguments, I think successfully. In Section 4.4 I briefly describe these test-theory arguments for distant simultaneity and I indicate why these arguments fail. My first task in the present section is to introduce the problem of distant simultaneity. I then discuss why debate over this problem has continued so long, why the problem cannot easily be dismissed, but also why I think the problem is not one of great significance.

Distant simultaneity relations specify which spatially separated physical events an observer will regard as occurring at the same time. The conventionality of simultaneity (CS) thesis, which is the thesis we are primarily concerned with here, pertains to distant simultaneity. According to the CS thesis, distant simultaneity relations in special relativity theory do not describe real physical relations which obtain between physical events, but are merely an artifact of our definitions. These relations cannot be determined empirically but only stipulated arbitrarily. The CS thesis is a controversial thesis. The problem of distant simultaneity is to determine whether or not this thesis is true.

The CS thesis contrasts with what we might call the relativity of simultaneity (RS) thesis, which is an essential and uncontroversial part of special relativity theory. According to the RS thesis, observers in relative motion will never agree about which events are simultaneous with one another. There is simply no objective, observer-independent simultaneity relationship in special relativity theory. This result follows from the fact that time in Einstein's theory is relative, meaning that the temporal duration between any two events in spacetime varies, albeit in a well defined way, from one inertial frame to another (see Section 3.3). It is not possible in special relativity theory to get relatively moving observers to agree on simultaneity relations merely by changing one's definitions. To assert that such agreement in fact obtains would be to relinquish special relativity theory in favour of another, empirically inequivalent theory, namely, Galilean kinematics.

The philosophers Hans Reichenbach [66] and Adolf Grünbaum [32] make a further "relativity" claim about special relativity theory, that goes beyond the familiar frame-dependent relativity of simultaneity. It is this claim which we know as the CS thesis. According to Reichenbach and Grünbaum there is not in Einstein's theory even an objective simultaneity relation within a frame. The basis of this claim is the purported absence, in an Einsteinian world, of any means of instantaneously synchronising spatially separated clocks. For, in such a world the fastest signal, light, travels at a finite speed.

The usual way of illustrating the conventionalists' point is to consider the temporal relations between two spatially separated clocks at rest with respect to one another and moving inertially. Suppose a light signal from Clock A is sent at time t_{A}, and arrives at Clock B at time t_{B}, thereupon the signal is reflected immediately back to Clock A, arriving at time t'_{A}. The simultaneity problem is to determine which reading of Clock A, between times t_{A} and t'_{A}, is simultaneous with t_{B}. If light propagates isotropically, then

$$t_{\mathrm{B}} - t_{\mathrm{A}} = t'_{\mathrm{A}} - t_{\mathrm{B}} \ , \tag{4.1}$$

which is equivalent to

$$t_{\mathrm{B}} = t_{\mathrm{A}} + \tfrac{1}{2}(t'_{\mathrm{A}} - t_{\mathrm{A}}) \ . \tag{4.2}$$

Equation 4.2 makes clear that the reading on Clock A which is simultaneous with t_{B} lies exactly half-way between t_{A} and t'_{A}. This is known as the standard, or Einstein, simultaneity relation.

But how do we know whether or not light propagates isotropically? To find out we need to determine the one-way speed of light in various directions. But to determine these speeds we must first synchronise spatially separated clocks, which in turn requires that we specify definite simultaneity relations for spatially separated events. Thus, the determination of simultaneity relations within a frame seems to require knowledge of the one-way speed of light, but to determine the latter we seem to need knowledge of the former.

According to Reichenbach and Grünbaum, this circularity problem is inescapable, which means we are free to choose any event between t_{A} and t'_{A}

on the worldline of Clock A as simultaneous with t_{B}. That is, we can always set

$$t_{\mathrm{B}} = t_{\mathrm{A}} + \epsilon(t'_{\mathrm{A}} - t_{\mathrm{A}}) \, , \tag{4.3}$$

where ϵ is any real number such that $0 < \epsilon < 1$. Choosing a non-standard value for ϵ (i.e. $\epsilon \neq \frac{1}{2}$) defines a plane of simultaneity which is tilted with respect to the worldline of Clock A, and serves to pick out a direction along which the speed of light is anisotropic. The existence of a distinguished spatial direction renders space itself anisotropic. Hence, the question of whether distant simultaneity relations in special relativity are objective is equivalent to the question of whether the one-way speed of light is objective, which in turn is equivalent to the question of whether the (an)isotropic character of space is objective.

One aspect of distant simultaneity relations which is important to the CS debate is their coordinate-dependent character. If, for example, in some inertial frame, with adapted coordinates (t, x, y, z), the distant simultaneity relation is assumed to be ϵ_1, then we can transform to another simultaneity relation ϵ_2 merely by a coordinate transformation:

$$\begin{aligned} \bar{t} &= t - 2(\epsilon_1 - \epsilon_2)x/c \, , \\ \bar{x} &= x \, , \\ \bar{y} &= y \, , \\ \bar{z} &= z \, , \end{aligned} \tag{4.4}$$

where c here is regarded as the round-trip speed of light. In the new coordinates $(\bar{t}, \bar{x}, \bar{y}, \bar{z})$ synchronised clocks set up throughout the frame read a time $2(\epsilon_1 - \epsilon_2)x/c$ less than clocks synchronised in the coordinate system (t, x, y, z). We can use Equation 4.4 to derive ϵ-dependent expressions for various physical quantities. For example, the speed of a body which leaves the origin at $\bar{t} = t = 0$ and arrives at $x = d$ at $t = d/v$ will be $\bar{v} = d/\bar{t} = v/[1 - 2(\epsilon_1 - \epsilon_2)v/c]$.

The coordinate-dependent character of distant simultaneity relations is relevant to my discussion in Section 4.4 of test-theory arguments for distant simultaneity. Before I discuss these arguments, however, I wish to make a few general remarks on the status and significance of the debate over the CS thesis. I will base my remarks on a comparison between the issue of distant simultaneity and the issue of spacetime geometry. This latter issue I will also examine more thoroughly in Sections 4.5 and 4.6.

Originally, Reichenbach and Grünbaum argued with as much vigor for the conventionality of geometry as for the conventionality of distant simultaneity. However, current philosophical debate over distant simultaneity relations is considerably more intense than debate over geometrical relations. I think that the reason for this difference, and for declining philosophical interest in the problem of geometry in particular, is due ultimately to formal differences in the theoretical contexts in which these two issues arise.

The problem of distant simultaneity arises, as we have seen, from the fact that ϵ appears not to be an empirically determinable quantity. It possesses, in fact, arbitrariness. Now it is true that in flat-spacetime gravity theory (whether Newtonian or Einsteinian) there are two quantities—a non-dynamical affine connection and a dynamical gravitational field—which, like ϵ, seem to possess arbitrariness. However, in gravity theory this arbitrariness can be removed by combining the affine connection with the gravitational field to form a new dynamical affine connection. This new connection describes the trajectory of bodies in curved spacetime. What is more, there exist some rather innocuous methodological principles (see Section 4.5) which tell us why we should remove the arbitrariness in flat-spacetime gravity theory, and, consequently, why we should accept the objectivity of curved spacetime.

The situation is different in relativistic kinematics. We cannot get rid of the arbitrariness in ϵ by combining it with some other arbitrary quantity, for there are no such quantities available in relativistic kinematics. Hence, it is more difficult to mobilise methodological principles to argue for determinate simultaneity relations than it is to argue for determinate geometrical relations. It is for this reason that I think the CS thesis is much more plausible than the conventionalist thesis about geometrical relations. It explains, I think, why debate over the CS thesis continues unabated today.

One of the more influential of recent arguments against the thesis has been advanced by the philosopher David Malament [50]. Malament shows how the standard simultaneity relation ($\epsilon = 1/2$) can be uniquely derived from the causal (i.e. conformal) structure of special relativity theory (together with some highly innocuous background assumptions). Malament's argument should impress philosophical relationalists about spacetime, because philosophical relationalists seek to keep their commitments to spacetime structure to a minimum, and in his argument Malament makes no allusion to metrical structure. He derives his conclusion from causal structure alone.[1]

However, for this very same reason Malament's argument will probably not impress most physicists. For most physicists, I suspect, are not philosophical relationalists. They do not believe in causal theories of time, according to which temporal relations should be reducible to, and therefore eliminable in favour of, causal relations. Consequently they do not shun metrical talk. However, Malament's argument presupposes causal theories of time, as he himself acknowledges. Malament's argument will not, I suspect, convince most physicists because his premises do not reflect all that physicists are committed to. In particular, these premises do reflect a com-

[1]Malament does state explicitly the Minkowski metric near the beginning of his paper, but he makes no further use of it. Following A. A. Robb [67], for example, Malament defines orthogonality to some worldline O, and hence the standard simultaneity relation Sim_O, not in metrical terms but purely in terms of the causal connectability relation and membership in O.

mitment to metrical structure.

By means of metrical structure, that is, by means of explicitly expressed metrical relations between physical events, one can pick out a direction of anisotropy, and thus introduce non-standard simultaneity relations. By abstaining from metrical talk Malament's argument, in effect, presupposes spatial isotropy. It is hardly surprising, therefore, that this argument leads to the conclusion that it does.[2] Defenders of the CS thesis can avoid Malament's conclusion by expressing a commitment to metrical relations over and above their commitment to causal relations. Making out this commitment (in, say, empirical terms) is another question, of course, but it is clear, even given Malament's argument, that the CS thesis cannot easily be dismissed.

While the CS thesis remains controversial (and for good reason), my own view is that the problem of distant simultaneity is not one of great significance when compared, say, to the less difficult problem of geometry. My reason is based on the fact that distant simultaneity relations, in contrast to geometrical relations, are not all that conditioning of other elements of theory. This fact is evident in the stability of our interpretations of physical quantities under simultaneity transformations. Velocities, for example, may have their magnitudes altered by a simultaneity transformation, but they remain velocities just the same. Other physical quantities (for example, momentum) do not even have their magnitudes altered. In contrast, changing our geometrical commitments has quite drastic consequences. On one set of geometrical commitments, for example, we interpret gravity as the action on matter of an inertial field. On another set of commitments, we interpret gravity as the manifestation of spacetime curvature.

Consider also the effect of changing our commitments on the field equations of special and general relativity, which currently express our most basic beliefs about spacetime. Different values of ϵ do not change one jot the form of these equations (which is no surprise really, given that these equations are generally covariant, and that ϵ-transformations can be effected simply by changing coordinates.) In contrast, different geometrical commitments make for striking and profound formal differences. These differences are strongly reflected in contemporary theorising about gravity. I said earlier that philosophical debate over the problem of geometry has declined. Nevertheless, in physics there is quite a divergence of opinion over what our geometrical commitments should be. Particle physicists argue, on heuristic grounds, that because flat spacetime has made possible the excellent quantum theories of matter we now possess, we should, consequently, seek a quantum theory of gravity which also is based on flat spacetime.[3] Opposed to this programme are cosmologists and general relativists (and, incidentally, myself) who argue against flat spacetime on both heuristic and more formal

[2] I owe to Philip Catton the criticism that Malament's starting points are too meagre to define anything but standard simultaneity.

[3] Rosen [70]; Gupta [35].

methodological grounds.

Cosmologists argue heuristically that it would be difficult to make sense of large scale phenomena (e.g. Hubble's Law) in terms of flat spacetime. General relativists argue more formally that flat spacetime is simply "unobservable" (see Section 4.5) and is therefore a bad theoretical entity.[4] In any case, differing beliefs about the geometrical structure of spacetime have led to radically different programmes for improving Einstein's theories. In contrast, debate over the status of distant simultaneity seems to have made no contribution to further theorising. It is for this reason above all that I think that the problem of distant simultaneity is not one of great importance.

4.4 Test theories for distant simultaneity?

By the lights of test-theory methodology, defenders of the CS thesis seem to have a strong case. For, they have selected an element of physical theory, the distant simultaneity relation, which they have parameterised in test-theory fashion, and they have shown (in my view) that there is no way of determining empirically the value of their test-theory parameter (ϵ). Indeed, they have shown that a phenomenon (the one-way speed of light) which would be relevant to determining the value of ϵ is in fact not empirically measurable antecedent to choosing some value of ϵ itself. Hence, from the perspective of test-theory methodology the thesis that distant simultaneity relations are conventional seems justified. It is surprising, therefore, that some physicists, while accepting for these reasons that distant simultaneity relations in special relativity theory cannot be determined empirically, nevertheless maintain that such relations are so determinable in the wider context of test theories for relativistic kinematics. In this section I will discuss these physicists' test-theory arguments for the measurability of distant simultaneity relations.

In one sense, of course, it is natural for physicists to look to test-theory constructions to see if they are able empirically to determine distant simultaneity relations. For one of the primary functions of test theories is to peg down empirically specific elements of physical theory. In the case of distant simultaneity relations, however, I believe that no test-theory argument to date convinces us that these relations are measurable or objective. Indeed, there are good reasons to believe that such arguments are not possible, given our current physical conceptions. Distant simultaneity relations, I will conclude, constitute one (albeit not very substantial) element of physical theory which at present is beyond the reach of test-theory methodology.

The two test theories for kinematics I discussed in Chapter 3 both presuppose standard ($\epsilon = 1/2$) simultaneity relations. In particular, Robertson's test theory presupposes these relations, despite Robertson's own claim to the contrary. Although Robertson assumes that light propagates isotropically in the laboratory frame he claims that his test theory makes "[n]o

[4] Deser [13].

assumption ... concerning the velocity of light or other physical law in [the boosted frame]". But about this he is mistaken. For he states explicitly, and in more than one place, that he has imposed Einstein synchronisation in the boosted frame, which entails that light will be observed to propagate isotropically in that frame. In any case, both Robertson's test theory and the other test theory for kinematics in Chapter 3 can be "generalised" so that they make no assumptions about distant simultaneity.

Mansouri and Sexl [52] have, for example, extended Robertson's test theory to allow for arbitrary simultaneity relations in the laboratory frame. However, they claim that this extension actually allows them to derive empirically a determinate simultaneity relation. According to Mansouri and Sexl, the following expression for the directional dependence of the one-way speed of light can be derived within their "generalised" test-theory framework:

$$c(\theta) = c - v(1 + 2\alpha)\cos\theta \ . \tag{4.5}$$

The constant c is here regarded as the round-trip speed and v is the speed of the laboratory frame relative to privileged frame of absolute rest, which Mansouri and Sexl refer to as the "aether" frame and identify as the rest frame of the cosmic background radiation. Since the test-theory parameter α (like v) is measurable, and moreover determined by observations to be $-1/2$, Mansouri and Sexl conclude not only that $c(\theta)$ is measurable, but that $c(\theta) = c$. Hence, the one-way speed of light, and, by implication, distant simultaneity relations, seem to be empirically determinable within Mansouri and Sexl's test-theory framework.

However, Vetharaniam and Stedman [81] have argued, cogently in my view, that Mansouri and Sexl's conclusion depends crucially on the fact that their test theory retains standard simultaneity relations in the aether frame. It is this fact, they argue, which makes Equation 4.5 possible in the first place. Hence, Mansouri and Sexl's test-theory argument for the isotropy of the speed of light ultimately is circular. Vetharaniam and Stedman [81] avoid Mansouri and Sexl's conclusion by further "generalising" the latter's test theory to include arbitrary simultaneity in the aether frame as well as in the laboratory frame. In their "more general" test theory, Equation 4.5, as it stands, cannot be derived.

Another test-theory argument for determinate simultaneity relations has been advanced by Clifford Will. Will claims that it is possible, within Manosuri and Sexl's test-theory framework, to measure directly the simultaneity parameter ϵ. Thus, Will's claim differs from Mansouri and Sexl's own claim that by using their test theory ϵ is determinable only indirectly. Nevertheless, Vetharaniam and Stedman [82] show how Will, in neglecting higher order terms in an expansion, has inadvertently eliminated the ϵ-dependency of one side of an equation, making it appear as though the ϵ-dependency of the other side can be determined empirically. On this basis Vetharaniam and Stedman reject Will's argument for the measurability of ϵ. Will's error serves to show that when we use test theories expressed in arbitrary simultaneity we must be careful to maintain ϵ-covariance when making approximations, otherwise mistaken interpretations can arise.

There is of course one very good reason why we should not be surprised that both individual physical theories and test theories can always be "generalised" to include arbitrary distant simultaneity relations. This is Kretschmann's [42] thesis that all dynamical laws in physics should be expressible in a general covariant (i.e. coordinate-independent) form. In Section 4.3 I pointed out how a transformation of distant simultaneity relations could be achieved merely by a coordinate transformation. General covariance of our theories ensures that it will always be possible to coordinatise physical events in a way which suggests distant simultaneity relations other than the standard relation.[5]

While this result appears to support the CS thesis, it has also been used to criticise that thesis. That the parameter ϵ can be fixed merely by choosing a coordinate system leads us to doubt its status as a genuine test-theory parameter. For, genuine test-theory parameters are determined empirically, by phenomena, not a priori, by coordinate choice. Arguably, the parameter ϵ does not after all parameterise some physical element of theory as do actual test-theory parameters. Rather, it seems that ϵ parameterises only a class of coordinate systems. Arguably, then, the mere possibility of arbitrary simultaneity formulations of physical theories (and test theories) does not by itself indicate anything about the physical (i.e. intrinsic) structure of spacetime. Michael Friedman argues along these lines when he suggests that defenders of the CS thesis need to provide a methodological argument, showing why arbitrary simultaneity formulations of physical theories are more "parsimonious" than standard simultaneity formulations, if the CS thesis is to amount to anything more than "simply a trivial consequence of general covariance".[6]

Given the conflicting claims about distant simultaneity surrounding the status of simultaneity transformations as coordinate transformations, it would be nice if there were an argument for the objectivity (or for the conventionality) of distant simultaneity relations, which is completely independent of coordinate-based considerations. In fact I have already discussed one such argument. This is David Malament's argument for the standard simultaneity relation. Malament's argument is based solely on causal connectability relations between physical events. These relations are coordinate-independent. If two events, described by some coordinate system, are causally connectable, then no coordinate transformation can destroy this

[5]Mark Zangari [91] has questioned recently the assumption that transformations to non-standard coordinates are always possible. He argues that one cannot adequately describe fermionic particles, like electrons, unless one adopts a spinorial representation of spacetime points. Because this representation fails to admit transformations to non-standard coordinates, Zangari concludes that the existence of fermionic particles empirically rules out the possibility of non-standard simultaneity relations. A critique of Zangari's argument is provided by Gunn and Vetharaniam [34].

[6]Friedman [29], pp. 175–176.

relationship. The coordinate-independent character of Malament's argument would seem to make it an especially powerful argument for the objectivity of standard simultaneity. However, coordinate-independence has come at a price for Malament. He has had to avoid in his argument referring to metrical structure, because invoking a metric explicitly would bind him to a specific coordinate system and thus force him to assume, arbitrarily, some value for ϵ. Defenders of the CS thesis can, as I pointed out in Section 4.3, avoid the conclusion of Malament's argument by making a commitment to metrical relations over and above their commitment to causal relations.

Whether Malament's argument secures for us determinate distant simultaneity relations depends ultimately on whether causal theories of time are true. In any case, it is clear that test theories for relativistic kinematics cannot secure for us these relations. The test-theory arguments for distant simultaneity I have described in the present section are faulty. They are based on misinterpretations and computational error. More generally, the parameter ϵ seems not to be a genuine test-theory parameter, for its value cannot be determined empirically. Consequently, test theories do not allow us empirically to condition that element of spacetime structure which is quantified by this parameter, if in fact there really is an element of physical spacetime structure which this parameter quantifies.

4.5 Arguments for curved spacetime

The PPN Formalism, which I discussed in Section 3.7, is an important modern test theory for gravitation. The basic equation of this test theory, Equation 3.18, is a generalised spacetime metric. Accordingly, the key conceptual presupposition of the PPN Formalism states that the correct theory of gravity is a metric theory, and most metric theories (though not all) imply that spacetime is curved. In the present section and the next I consider what warrant there is both for the proposition that gravity is metrical in character and for the proposition that spacetime is curved. Specifically, in the next section, I consider whether it is possible to run a test-theory-type argument for metricality. I conclude that such an argument is not possible, strictly speaking, and that less formal methodological considerations in combination with test theories are required cogently to argue for the metricality of gravity, and to argue for the curvature of spacetime. In the present section I introduce the problem of spacetime geometry, and I discuss the methodological principles which are needed to solve this problem.

Geometrical relations concern, among other things, the distance and temporal duration between physical events. They also tell us, among other things, which lines (trajectories) are the straightest (inertial) ones, and what angle one such line makes with another which it intersects. The conventionality of geometry (CG) thesis states that geometrical relations are not real physical relations, but are merely an artifact of our definitions. According to this thesis, which has been defended by Reichenbach and Grünbaum, among others, geometrical relations cannot be determined empirically, but must be stipulated arbitrarily. The problem of geometry is to determine whether or not the CG thesis is true. In the twentieth century this problem has become inseparably linked with gravity theory, due to Einstein's demonstration that gravity can be described in a way which involves matter conditioning the geometry of four-dimensional spacetime. In gravity theory the problem of geometry not only receives a very explicit formulation, but also admits, in my view, of a rather straightforward solution. In this section I begin by presenting a theory of gravity in flat spacetime. I then direct the reader's attention to features of this theory which allow it to be reformulated in terms of curved spacetime. Finally, I discuss why the curved-spacetime formulation is preferable to the flat-spacetime one.

Although the idea of curved spacetime first arose in a relativistic context, this idea is not in fact peculiar to relativistic analyses of gravity. Élie Cartan [9] showed this in 1923 when he indicated how to reformulate Newtonian gravity theory (NGT) as a curved spacetime theory. Cartan's result means that the essential features of gravitation which make possible a move to curved spacetime can be examined in the more familiar context of classical physics. A full relativistic analysis is not required. Indeed, philosophers who have argued for the truth of curved spacetime typically have presented their case with NGT in view. I will do the same thing here, though in Section 4.6 I will consider the possibility of a test-theory argument for curved spacetime which is independent of any particular theory of gravity.

The possibility of reformulating standard NGT as a curved-spacetime theory is due partly to a theoretical feature of NGT and partly to an independent empirical result. The theoretical feature is the arbitrariness of the Newtonian scalar potential field.[7] This arbitrariness is evident in the field equation of flat-spacetime NGT, Equation 3.1, which in general covariant form may be written

$$h^{ij}\nabla_i\nabla_j\Phi = 4\pi G\rho \text{ .}^{8} \qquad (4.6)$$

Equation 4.6 remains unchanged in form under the field transformation $\Phi \to \Phi + \mathbf{a}\cdot\mathbf{x}$, where $\mathbf{a} = \mathbf{a}(t)$ is an arbitrary function of time. This means that in the absence of sufficiently strong boundary conditions Φ is not determined uniquely by the local matter distribution ρ. It is true that in particular models of this theory, such as a single-source, or "island", universe, one can impose boundary conditions (e.g. $\Phi \to 0$ as $\mathbf{x} \to \infty$) which determine Φ uniquely. But in general, where there is a finite distance between one source and the next, Φ will be indeterminate.[9]

[7]There exists a formulation of NGT in which the gravitational field is not a scalar but a vector field. (See, for example, Doughty [16], p. 141.) This vector field is sometimes called a 'field strength', to denote its lack of arbitrariness. However, a careful analysis shows that this field still possesses sufficient freedom to permit the move to curved spacetime.

[8]The h^{ij} are the contravariant components of the Euclidean spatial metric, while the ∇_i constitute the spatial part of a flat 4-D affine connection compatible with this metric.

[9]Glymour [30], pp. 240–241.

The equation of motion for a particle moving along a trajectory $x(t)$ under the influence of Φ is

$$m_{\mathrm{I}}\left(\frac{d^2x^i}{dt^2}+\bar{\Gamma}^i_{jk}\frac{dx^j}{dt}\frac{dx^k}{dt}\right)=-m_{\mathrm{G}}h^{il}\nabla_l\Phi\ .^{10} \tag{4.7}$$

The quantities m_{I} and m_{G} are, respectively, the inertial mass and the (passive) gravitational mass of the particle. These two quantities are, in flat spacetime, conceptually distinct. The inertial mass is a measure of the extent to which the particle's velocity changes in response to an impressed force. The gravitational mass is a measure of the strength with which the gravitational field couples to the particle.

While m_{I} and m_{G} are (in flat spacetime) conceptually distinct, they are nevertheless empirically equivalent. That is, for all bodies within the reach of experiments it has been found that $m_{\mathrm{I}} = m_{\mathrm{G}}$, a result which is widely referred to as *the equivalence of inertial and gravitational mass.* This is the independent empirical result which makes possible a reformulation of NGT in terms of curved spacetime. It allows us to rewrite Equation 4.7 as

$$\frac{d^2x^i}{dt^2}+\left(\bar{\Gamma}^i_{jk}+h^{il}\nabla_l\Phi t_j t_k\right)\frac{dx^j}{dt}\frac{dx^k}{dt}=0\ , \tag{4.8}$$

where $t_j = \partial t/\partial x^j$.

Now the affine connection ∇ specifies which trajectories are the straightest, or inertial, trajectories. The vanishing of the connection coefficients $\bar{\Gamma}^i_{jk}$ on some trajectory is sufficient for that trajectory to be inertial. However, according to Equation 4.8, we cannot use the flat-spacetime formulation of NGT to distinguish inertial from accelerated trajectories. The reason is that in general we cannot determine separately the components $\bar{\Gamma}^i_{jk}$ of the affine connection or the components $h^{il}\nabla_l\Phi$ of the gravitational field gradient. Only the combination of these quantities may be determined empirically. For, on any trajectory we can make disappear non-zero components of $\bar{\Gamma}^i_{jk}$ (which do not arise from rotation) by absorbing them into the gravitational gradient term.

To see this, suppose that in some frame we find that (1) $\bar{\Gamma}^i_{00} = a^i$, (2) all other $\bar{\Gamma}^i_{jk} = 0$, and (3) Φ_1 is a solution of the field equation sourced by some matter distribution ρ. Because $\bar{\Gamma}^i_{00} \neq 0$ our frame appears to be an accelerated frame. However, with impunity we can set $\bar{\Gamma}^i_{00} = 0$ by making the field transformation $\Phi_1 \rightarrow \Phi_2 = \Phi_1 + a^i x_i$. We can do this because Φ_2 is also a solution of the same field equation, due to the arbitrariness in the gravitational field. Now our frame looks like an inertial frame. Thus, flat-spacetime NGT appears to support the idea (or principle) of general relativity, according to which the distinction between uniform and (linearly) accelerated motion is impossible to make out.

It is crucial to recognise that we have arrived at this result only because (1) Φ possesses arbitrariness, and (2) $m_{\mathrm{I}} = m_{\mathrm{G}}$ jointly obtain. If, to the

[10] The $\bar{\Gamma}^i_{jk}$ are the connection coefficients associated with ∇.

contrary, Φ was uniquely determined by the field equation, then there would be no way of absorbing non-zero components of the affine connection into the gravitational field term of Equation 4.8. Hence, there would be no way of rendering inertial a frame which initially looks accelerated (and vice versa). On the other hand, if the ratio $m_{\rm I}/m_{\rm G}$ differed, say, for particles of varying chemical constitution, then there would be no way of uniquely redefining Φ in order to absorb non-zero connection components. One would need a different Φ for each value of $m_{\rm I}/m_{\rm G}$.[11] Thus, the arbitrariness that we find in the affine connection is due ultimately to the equivalence $m_{\rm I} = m_{\rm G}$.

It is the arbitrariness of both the gravitational potential field and the affine connection which makes possible the reformulation of standard NGT as a curved-spacetime theory. This reformulation involves combining these two arbitrary quantities to form a new and unique affine connection

$$\Gamma^i{}_{jk} = \bar{\Gamma}^i_{jk} + h^{il}\nabla_l\Phi t_j t_k \ , \tag{4.9}$$

which allows us to rewrite Equation 4.8 as

$$\frac{d^2x^i}{dt^2} + \Gamma^i{}_{jk}\frac{dx^j}{dt}\frac{dx^k}{dt} = 0 \ . \tag{4.10}$$

The field equation corresponding to this reformulation of Newtonian gravity theory can be obtained by substituting Equation 4.9 into the standard expression $R_{jk} = \partial_k\Gamma^l{}_{jl} - \partial_l\Gamma^l{}_{jk} + \Gamma^m{}_{jl}\Gamma^l{}_{mk} - \Gamma^m{}_{jk}\Gamma^l{}_{ml}$, from differential geometry, and comparing the result with Equation 4.6.[12] One important way in which this curved-spacetime formulation of NGT differs from the flat-spacetime one is that its unique affine connection enables us clearly to distinguish actual accelerated from actual unaccelerated motion.

In the context of NGT the problem of spacetime geometry becomes the problem of determining whether or not one of our two formulations of NGT is objective. Because it can be derived from flat-spacetime NGT, curved spacetime NGT must be empirically equivalent to its flat-spacetime counterpart. Hence, it is not possible to choose between the formulations merely on the basis of what predictions they make. What is needed is some kind of methodological argument which tells us which one of the formulations is more parsimonious than the other, and which thereby allows to ascribe to spacetime either a flat or a curved geometrical structure.

Michael Friedman has put forward such an argument. Friedman prefers curved-spacetime NGT, on the basis that reference frames distinguished theoretically by flat-spacetime NGT "are actually equivalent or indistinguishable".[13] Flat-spacetime NGT distinguishes theoretically between uniform and accelerated motion because it posits an affine connection. However,

[11]This state of affairs actually obtains in electromagnetic theory, where the electromagnetic field possesses a similar sort of arbitrariness to Φ, but the ratio $q/m_{\rm I}$ differs from one particle to another.

[12]The complete curved-spacetime formulation of NGT is given by Friedman [29], p. 101.

[13]Friedman [29], pp. 121–122.

because this connection is not unique, as we have seen, we cannot use the theory to tell whether actual bodies are moving uniformly or whether they are really accelerating. Thus, according to Friedman, flat-spacetime NGT is inferior because it draws a theoretical distinction which corresponds to no observable difference. Curved spacetime NGT does not do this because it possesses a unique affine connection which does allow us to decide whether bodies are moving inertially or not.

Clark Glymour's [30] earlier argument for curved spacetime, is similar in substance to Friedman's argument, but is expressed in the language of confirmation theory. Glymour's main charge is that flat-spacetime NGT is less well tested by the evidence than is curved-spacetime NGT. Glymour does not mean by this that the two formulations are empirically inequivalent, and that one formulation has fewer successful predictions than the other. Glymour accepts the empirical equivalence of the two formulations, but maintains nevertheless that flat-spacetime NGT contains a hypothesis which is untestable. (Testability, for Glymour is synonymous with bootstrap determinability, which I shall discuss in Section 5.8.) This hypothesis states that the motion of a body under gravity is due to both an inertial and a non-inertial part, that is, to both $\bar{\Gamma}^i_{jk}$ and $h^{il}\nabla_l\Phi$, respectively. We have seen, however, that only the combination of these two parts is empirically determinable. Since curved-spacetime NGT does not contain this untestable hypothesis but is otherwise like standard NGT, curved spacetime NGT is, according to Glymour, the better confirmed of the two formulations, and therefore more likely to be true.

In my view these methodological considerations enable us decisively to solve the problem of geometry in the context of classical physics. For these considerations provide good reason for preferring curved to flat spacetime, and thus for rejecting the conventionality of geometry (CG) thesis. These considerations are, I believe, what some gravitation theorists have tacitly in mind when they spurn flat spacetime because it is "unobservable". [14] However, one shortcoming of Glymour's and Friedman's analyses is their failure explicitly to link the possibility of curved spacetime to the equivalence of inertial and gravitational mass, as I have done above. Indeed, their arguments give no indication that curved spacetime has anything to do with this important empirical result. In contrast, Clifford Will's test-theory-based argument for curved spacetime, which I shall examine in Section 4.6, keeps the equivalence result in full view. What Will's argument lacks, however, is the methodological insight of Friedman and Glymour, which is required to make his argument cogent.

4.6 Test theories for geometry?

In this section I consider the possibility of test theories for curved spacetime in relativistic physics. The problem of geometry in this context is

[14] Deser [13].

significantly different from the analogous problem in classical physics. In classical physics metric theories of gravity are not possible, because a four-dimensional spacetime metric is not definable there. Curved-spacetime NGT, for example, is not a metric theory.[15] Metric theories of gravity are of course possible in relativistic physics, but what is really striking is that not all metric theories are curved-spacetime theories. In some metric theories of gravity spacetime is flat (or constantly curved) and non-dynamical.

The PPN Formalism in fact represents metric theories of both types. General relativity, for example, is a curved-spacetime theory. But there are also bimetric theories, stratified theories and conformally flat theories, all of which contain both a dynamical metric field, to which matter responds, and a flat (or constantly curved), non-dynamical, background metric for spacetime.[16] These theories may seem odd, but they are consistent, and what is more they are not empirically equivalent to other metric theories like general relativity theory. Hence, the existence of the PPN Formalism shows clearly that test theories for spacetime geometry in relativistic physics are possible. Of course, they are possible, only given that the presupposition of metricality is possible. What I want to discuss in this section is the warrant for this presupposition.

According to Clifford Will, there are phenomena which "accurately verify that gravitation is a phenomenon of curved spacetime, that is, it must be described by a 'metric theory' of gravity". [17] Although Will suggests at this point that he will provide support for the more narrow thesis that spacetime is curved, his actual argument concludes directly for the broader thesis that gravity is metrical in character.

In outline, Will arrives at this conclusion as follows. First, he distinguishes between two equivalence principles, which he calls respectively the 'Weak Equivalence Principle' (WEP) and the 'Einstein Equivalence Principle' (EEP). WEP is a necessary, but not sufficient, condition for EEP.[18] Will then shows how by using a test-theory-type construction one can derive WEP from phenomena. Will shows also how one can derive from phenomena, again using test-theory constructions, further results which, taken together with WEP, yield EEP. Finally, Will claims that "it is possible to argue convincingly that if EEP is valid [i.e. true], then gravitation must be a curved-spacetime phenomenon".[19]

Let us take a closer look at the individual steps in Will's reasoning. WEP states that for any body its inertial mass m_I be equivalent to its

[15]It is for this reason that a test theory which represents both NGT and general relativity theory is not possible.

[16]Will [87], pp. 130–141.

[17]Will [87], p. 10.

[18]There is a conjecture, known as 'Schiff's Conjecture', which states that "any complete, self-consistent theory of gravity that embodies WEP necessarily embodies EEP". This conjecture, however, has been proved rigorously only for a narrow class of theories—see Will [87], p. 38ff.

[19]Will [87], p. 22.

(passive) gravitational mass m_{G}. Will points out that the inertial mass of a typical laboratory body is made up of several forms of energy, including "rest" energy, electromagnetic energy, weak-interaction energy etc., and he considers the possibility that one or more of these forms might contribute differently to m_{G} from how they contribute to m_{I}. Thus, Will writes

$$m_{\mathrm{G}} = m_{\mathrm{I}} + \sum_{\mathrm{A}} \eta^{\mathrm{A}} E^{\mathrm{A}}/c^2 \ , \tag{4.11}$$

where E^{A} is the energy form generated by interaction A, and η^{A} quantifies the strength of WEP violation due to A. Evidently, this construction is a simple test theory, and Will goes on to show how phenomena observed in modern high-precision Eötvös-type experiments place stringent limits on the size of the test-theory parameters η^{A}.[20]

WEP implies that an observer in free fall towards a gravitational source will see neutral test bodies close by move as if there were no force on them (neglecting tidal effects). As Will puts it, Einstein extended WEP by proposing that for such an observer not just mechanical but all the special relativistic laws of physics would obtain as if gravity were absent. Einstein's supposed extension of WEP yields EEP, which, evidently, is stronger than WEP. Indeed, EEP can be regarded as a conjunction of WEP and a statement to the effect that special relativity obtains in freely falling frames. Will actually divides up the relativity part of EEP into two further parts, one of which he calls Local Lorentz Invariance (boost invariance) and the other which he calls Local Position Invariance, and considers the empirical warrant for these conditions individually. Since we can provide test-theory constructions both for WEP (as above) and for special relativity (see Sections 3.4 and 3.6) it is clearly possible to deduce EEP from phenomena.

What we now want to know is whether this deduction of EEP amounts to a deduction from phenomena of the metrical character of gravity. Will's argument to this conclusion involves first establishing the existence of a preferred family of frames, the freely falling frames, in which the spacetime metric is the Minkowski metric, and then showing that this state of affairs obtains only if there exists globally a unique non-Euclidean metric. My view is that Will's argument, as it stands, is not convincing. For, at the very beginning of his argument Will states that "WEP endows spacetime with a family of preferred trajectories, the worldlines of freely falling test bodies." However, this premise, which is crucial to the cogency of Will's argument, is surely false. For, in Section 4.5 we saw how even in the context of a specific theory of gravity (flat-spacetime Newtonian gravity theory) WEP actually helped make it impossible for us to tell which bodies were really in free-fall. There, WEP, in combination with the arbitrariness of the gravitational potential field, allowed us to convert any linearly accelerating frame into an inertial frame and vice versa. If in the context of some particular theory WEP does not pick out for us a family of privileged (i.e. inertial) frames or

[20] Will [87], pp. 24–29.

trajectories, then Will's claim that WEP alone can pick out for us a family of such frames cannot be true.

Indeed, it is a conceptual presupposition of Will's test-theory construction for WEP that gravity is non-metrical. For, only in non-metrical theories can one distinguish conceptually, as Will does, between inertial and gravitational mass. What is more, Will makes it clear that the starting point for his argument is the Dicke framework, according to which "[s]pacetime is a four dimensional differentiable manifold ... [which] need not a priori have either a metric or affine connection".[21] However, to discuss theoretically the possibility of privileged (i.e. inertial) trajectories one needs an affine connection defined on spacetime. What is more to discuss actual trajectories of this kind one needs a unique affine connection. Metrical theories of gravity, like general relativity, possess such a connection, but flat-spacetime non-metrical theories which respect WEP do not. Thus, I fail to see how WEP alone could enable us to distinguish a class of inertial trajectories.

Neither can EEP help us out here, for EEP's second condition, that special relativity holds locally, does not imply the existence of a unique global connection. Thus, EEP is not sufficient to ensure that gravity is a metrical phenomenon. EEP thereby fails to guarantee that spacetime is curved. Something extra must be added to EEP to obtain these results. This something extra must at the very least stipulate the existence of an affine connection, and moreover refer to methodological principles which tell us why gravity theories with unique connections are the preferable ones. The principles we need in fact are just the ones used by Glymour and Friedman to argue for curved spacetime in the context of Newtonian gravity theory (see Section 4.5).

In conclusion, test-theory-type constructions by themselves are not sufficient to convince us that gravity is metrical in character. For, while test-theory arguments make it possible to deduce from phenomena both WEP and EEP, methodological considerations must be brought to bear at some point if we are to use these equivalence principles to warrant the proposition that gravity is metrical. This case is somewhat analogous to the case of Newton and universal gravitation. A test theory enabled Newton to provide strong empirical support for the proposition that certain celestial forces are of an inverse-square character. But to infer from this proposition that the forces are gravitational in nature, and to infer moreover that all bodies interact gravitationally by means of inverse-square forces required Newton to employ his Rules of Reasoning. These rules are, as we have seen, methodological principles which concern the rationality of induction and the connection between theoretical simplicity and truth. When Newton tells us, in Rule 1, not to posit more causes than are needed to explain phenomena

[21]Will [87], p. 17. The Dicke framework, as it is called, is presented in Appendix 4 of Dickes article "Experimental Relativity", in DeWitt [14], pp. 163–313. This framework is not itself a test theory so much as it is a set of mathematical and theoretical assumptions that any physical theory, and thus any test theory, of gravity should satisfy.

he is telling us something similar, I think, to Friedman, who counsels us not to draw theoretical distinctions which make no observable difference. In my view, Einstein, in reasoning his way cogently to curved spacetime, also surely had something like Newton's Rules tacitly in mind.

All of these cases highlight I think the fact that test theories have their limits, and that more general, though less formal, methodological principles must brought to bear at some stage, if the problem of underdetermination at the deepest level is to be overcome. It is true, nevertheless, as I emphasised especially in regard to Newton, that test-theory constructions provide the kind of robust, empirically-based results that allow physicists confidently to apply these more general methodological principles.

4.7 Efficient versus final cause?

In relativity theory, and even more so in quantum theory, the Newtonian concept of 'force' has little purchase. None of the test theories for relativity I examined in Chapter 3 make any use of this concept. In fact, the concept does not have to prevail even in Newtonian physics. In Section 3.2 I showed how one could construct a test theory for Newtonian gravity in which the concept of 'field', rather than that of 'force', was central. Here, I present another test theory for classical physics, one which involves neither fields nor forces. Instead of Newton's dynamical principles, this test theory presupposes William Hamilton's Principle of Stationary Action.

I argue in this section that the possibility of test theories for action (as opposed to force) is important for our understanding of causation. This possibility, I argue, makes it difficult for us to decide whether efficient or final causes objectively describe fundamental physical interactions. I acknowledge, moreover, that the existence of Hamilton's Principle challenges the very notion of an efficient/final cause dichotomy in mechanics, and I speculate that a more formal conception of cause, based on principles which are often used in conjunction with Hamilton's, might be able to transcend this dichotomy.

Seventeenth century mechanical philosophers rejected as unintelligible Aristotle's animistic idea that natural things possess an internal origin of change. In so doing, they rejected his concomitant notion of 'final cause'. In particular, they rejected Aristotle's understanding of bodily motion as arising from occult, goal-directed propensities hidden within bodies, as I pointed out in Section 2.6. Mechanical philosophers admitted only efficient causal explanations for motion. On this basis, for example, they developed an aether theory, according to which both (terrestrial) gravitational motion and the motions of planets arise purely mechanically, through the action of push-contact forces.

The abstract character of Newton's dynamical conceptions makes them look animistic relative to the corresponding conceptions of orthodox mechanical philosophy—a difference which was keenly felt by Newton's contemporaries in science, and by Newton himself. Yet, Newton's conceptions do retain something of the efficient causal character of more traditional mechanical conceptions. For, according to Newton a body's change of motion (though not motion per se) is due to forces impressed upon the body from without, rather than to some internal propensity. Indeed, we might even consider the success of Newton's dynamical conceptions as evidence for the objectivity of efficient (as opposed to final) causation. We might, that is, if Newton's conceptions had remained unchallenged in classical physics. Such was not to be the case, however.

Leibniz, a mechanical philosopher, eventually "returned to animism" even more strongly than Newton. Leibniz's notion of *living force* (roughly, kinetic energy), which even Newton rejected, was the source for some further remarkable developments in mechanics, due mainly to Euler and Lagrange. These developments culminated in Hamilton's dynamical Principle of Stationary Action. In classical physics the mathematical abstractness, sophistication and power of Hamilton's Principle matches Newton's own dynamical conceptions, yet this principle possesses not the efficient causal overtones of Newtonian mechanics but the final causal overtones of occult quantities.

Qualitatively, the difference between Newton and Hamilton concerns how they explain the evolution of a physical system. Newton divides up the system into parts and considers the moment by moment evolution of each part due to its interaction with the other parts which are regarded as external to it. Hamilton, in contrast, does not partition the physical system, but considers it as a whole. According to Hamilton, the entire system evolves in such a way as to make an internal property of it—its action—stationary. There is no equivalent of Newton's First or Third Laws of Motion in Hamilton's dynamics, because both these laws presuppose distinctions between external and internal quantities which are irrelevant to Hamilton's description. (Newton's First Law presupposes a distinction between inertia and impressed force, while Newton's Third Law, presupposes a distinction between acting and being acted upon.) What is more, in Hamilton's formulation later states of the physical system condition the system's earlier evolution. It is this feature, together with the internal, or "occult", character of action, which gives Hamilton's Principle of Stationary Action its teleological flavour.

Mathematically, Newton's dynamical principles (i.e. his three laws of motion) can be represented by a set of differential equations, whereas Hamilton's Principle of Stationary Action can be represented by a single integral equation:

$$\delta \int_{t}^{t'} L dt = 0 \ . \tag{4.12}$$

The *Lagrangian* L is an energy function of coordinates and velocities. It encapsulates all the dynamical information of a system making it, to some extent, analogous to the force appearing in Newton's Laws of Motion. The symbol δ represents not the derivative (of some quantity) along a "path" in configuration space but the variation (of some quantity) between a point on one path and the corresponding point on a path adjacent to it. In words Equation 4.12 states that *a physical system evolves from its configuration at time t to its configuration at later time t' in such a way that the variation of the action $\int Ldt$ between the path taken and a neighbouring virtual path, with the same fixed end points, is zero (i.e. the action is stationary).*

Performing explicitly the variation in Equation 4.12 yields a set of differential equations—the so-called Euler-Lagrange Equations—which are necessary and sufficient conditions for Equation 4.12 to obtain. For a system of j particles moving along trajectories $\mathbf{z}_j(t)$ with velocities $\dot{\mathbf{z}}_j(t)$, the Euler-Lagrange Equations are:

$$\frac{d}{dt}\left(\frac{\partial L}{\partial \dot{\mathbf{z}}_j}\right) = \frac{\partial L}{\partial \mathbf{z}_j} \ . \tag{4.13}$$

In this particular case $L = K - V$, where K is the total kinetic energy of the particles and V is a scalar potential. The Euler-Lagrange Equations enable us to construct test theories for L, and thus for the action $\int Ldt$. They allow us to establish, for example, an equivalence relationship between a Lagrangian and the period-radius relation for a particle travelling with tangential velocity $v = R\dot{\theta}$ on a circular orbit in a central potential:

$$L_c \propto \tfrac{1}{2}v^2 + \frac{1}{2nR^{2n}} \Longleftrightarrow T \propto R^n \ . \tag{4.14}$$

This equation is directly analogous to Equation 2.2, which pertains to Newton's test theory. However, Equation 4.14 is based not on Newton's dynamical principles, involving forces, but on Hamilton's Principle of Stationary Action. Similar test theories, also based on Hamilton's Principle, can be constructed for the Newtonian gravitational field and for relativistic theories of gravity. Hence it is possible to construct test theories for physical systems that seem to avoid altogether connotations of efficient causation.

This result presents us with a dilemma. Which dynamical framework should we regard as physically objective, Newton's or Hamilton's? In Section 2.6 I concluded that the possibility of test theories for force implied that we should regard Newtonian forces as representing real physical relations between bodies. Should we now regard Hamiltonian actions in the same way? Can we, indeed, be committed simultaneously to the objectivity of both efficient and final causation? Or should we rather adopt a conventionalist view of these dynamical conceptions?

The philosopher Roger Jones [40] is, like me, interested in the metaphysical implications of principles such as Hamilton's. Although Jones states (wrongly, in my view) that "the approach in terms of a minimum principle seems to have no connotations of causality", he is clearly concerned about the bearing of this principle on the issue of scientific realism. Alan Musgrave [56], in reply to Jones, tries to run a Lakatosian line in favour of efficient causal notions. He seems to regard such notions as constitutive of a progressive research programme, while final causal notions are constitutive of a degenerating one. However, it is clear from his article that Musgrave is aware neither of the historical development, nor of the sophistication and power, nor of the predominance in twentieth century physics, of Hamilton's Principle.

Unlike Musgrave, Yourgrau and Mandelstam [90] have a deep appreciation of the importance for physics of Hamilton's Principle. What they object to are teleological interpretations of this principle. Their objections are based primarily on the abstract mathematical character of the principle, and the fact that it is not a principle of "minimum" but of "stationary" action. I agree that in its mathematical character Hamilton's Principle is as far removed from the anthropomorphic occult quantities of Aristotle as Newton's Laws are removed from the more "intelligible" push-contact mechanisms of the mechanical philosophers. Yet, in light of the observations I have already made, it seems mistaken to deny that either Hamilton's or Newton's principles retain something of the causal character of previous, less sophisticated dynamical conceptions.

It is true also that Hamilton's Principle is not merely a principle of least action, and is thus distinguished from various minimum principles once believed to have theological or mystical significance. Nevertheless, what is pertinent to the teleological or "occult" character of this principle is not the obtaining of minima but that by its lights physical systems evolve towards future states always in a way which guarantees that their defining internal property (action) satisfies a specific condition (stationarity). It is the internal nature of action and its relationship to future states, not the obtaining of minima, which gives Hamilton's Principle its teleological flavour.

In my view, one thing that is wrong with the debate over the status of efficient and final cause conceptions is that it tends to pit the two sets of conceptions against one another. Yet, analysis of Hamilton's Principle shows that in mathematical physics these conceptions are closely linked. Richard Feynman explains this link as follows:

> Now if the entire integral ... is a minimum [or maximum], it is also necessary that the integral along the little section from a to b is also a minimum. It can't be that the part from a to b is a little bit more. Otherwise you could fiddle with just that piece of path and make the whole integral a little lower. So every subsection of the path must also be a minimum. And this is true no matter how short the subsection ... Now if we take a short enough section of path how the potential varies from one place to another far away is not the important thing ... The only thing you have to discuss is the first order change in the potential ... So the statement about the gross property of the whole path becomes a statement of what happens for a short section of path—a differential statement.[22]

[22]Feynman [28], p. 19-8.

Feynman's "differential statement" is just what is constituted by the Euler-Lagrange Equations. For example, the Euler Lagrange equation pertaining to our test theory for action in Equation 4.14 is $a \propto 1/r^{2n-1}$ (where a is acceleration). Apart from the fact that it does include forces explicitly, this equation is identical to the basic equation of Newton's test theory.

I believe that the existence of a mathematical connection between the integral and differential laws of mechanics indicates that it is misguided to believe in a deep and thorough-going opposition between efficient- and final-cause conceptions, at least in mathematical physics. Philip Catton has suggested that these two formulations of mechanics have in common features which motivate a more formal conception of cause which transcends the efficient-final cause dichotomy. In my view, principles such as Noether's theorem and the Gauge Principle, which are often used in conjunction with Hamilton's Principle, point to such a formal account.[23] Both these principles concern the dynamical symmetries of physical systems, and the Gauge Principle in particular provides a prescription for using these symmetries to derive from the laws of non-interacting physical systems the laws of interacting systems. In this way the Gauge Principle explicitly links causation to symmetries, that is, to purely formal properties of physical systems which have no efficient- or final-causal overtones.

Pending a more formal account of causation, based on dynamical symmetries, what is clear at present is that neither test-theory methods, nor more general methodological considerations, allow us to decide in favour of either efficient- or final-cause conceptions in mechanics. One reason for this, I think, is simply that these conceptions are not mutually incompatible, but are in fact closely linked.

4.8 Test theories and quantum physics

All the test theories I have so far discussed in this dissertation have been test theories for kinematics and gravitation. As far as I know, the term 'test theory' is only ever applied by physicists working in either of these two fields. Yet, we might well ask whether it is possible to construct test theories for dynamical theories other than gravitation theory, such as classical electromagnetic theory and quantum mechanical theories of matter. A simple test theory for classical electromagnetism could be obtained by generalising Maxwell's field equations in a way directly analogous to our generalisation, in Section 3.2, of the field equation for Newtonian gravity. For example, we might add to the covariant second-order vacuum equation $\Box A - \partial\partial \cdot A = 0$ a term linear in the electromagnetic 4-vector potential A. The coefficient of this term would parameterise the rest mass of propagating electromagnetic waves, and the resulting test theory could be used to determine empirically the value of this mass from electromagnetic wave phenomena.

[23] For a discussion of these principles, see Doughty [16], pp. 189–201, 447–449.

Jon Dorling [15] has shown how Einstein's 1905 argument for the particulate, or quantum, character of radiation can be reconstructed as a Newtonian deduction from phenomena. Dorling's result indicates that Newtonian methods are not entirely alien to quantum physics, despite the considerable gulf which exists between classical and quantum physical conceptions. In the present section I ask whether test theories for quantum mechanics are possible. I put this question first to specific dynamical theories within the quantum mechanical framework, and secondly to the basic principles of quantum mechanics itself.

The principles of relativistic quantum field theory (RQFT) constitute the dynamical framework of the most successful theories of matter to date. Feynman's Path-integral Quantisation Principle has proved particularly valuable in the construction of successful quantum theories of specific material interactions, such as the electrodynamic and nuclear interactions. Feynman's Principle is in fact based on the existence of a "classical" Lagrangian L obeying Hamilton's Principle of Stationary Action. For the non-relativistic case without spin, path-integral quantisation states that the "transition" probability $\langle q'|q\rangle$ for a particle at q at time t to be at q' at later time t' is given by

$$\langle q'|q\rangle = \int \mathcal{D}q e^{\frac{i}{\hbar}\int_t^{t'} L(q,\dot{q})dt} , \tag{4.15}$$

where the integration is taken over all "paths" in configuration space. Thus, while Hamilton's Principle states that classical systems evolve along just one path (the stationary one), quantum systems seem, bizarrely, to evolve simultaneously along all paths, though the appearance of the classical action in Feynman's path-integral ensures that the stationary path will be the strongest contributor to the transition probability.

Specifying a particular Lagrangian L specifies the quantum theory relevant to some particular interaction. In RQFT one such theory is Quantum electrodynamics (QED), which describes how charged particles with spin interact electromagnetically via a dynamical quantum field. The QED Lagrangian contains the Dirac field, representing charged particles of half integer spin, and the 4-vector potential A, representing the (initially unquantised) electromagnetic field. However, many other Lagrangians can be constructed. We saw, in the last section, how it was possible to construct a classical test theory based on Hamilton's Principle by parameterising the distance-dependence of a Lagrangian describing particle motion in a central potential. This kind of procedure should also be possible in RQFT. Given that it is possible, the test-theory parameters which appear in the Lagrangians of RQFT will appear, derivatively, in the transition probabilities calculated from such Lagrangians, via Feynman's path integral scheme. The numerical values of these parameters could then be determined empirically by comparing the calculated transition probabilities with the kind of stochastic phenomena observed in quantum mechanical systems. Thus, test theories would be possible in RQFT, just as they are possible in nonquantum relativity theory and in Newtonian physics.

We also want to know whether it is possible to provide a test theory not just for specific quantum mechanical theories, like QED, but for the quantum mechanical framework itself. Historically, the desire for an alternative (but empirically adequate) theory to traditional quantum theory has been motivated primarily by philosophical worries about this theory's radical implications for our understanding of causality, and for the reality of a physical world existing independently of us. These worries have been expressed ever since quantum mechanics' inception, and even of course by Einstein himself. Einstein [25], along with Podolsky and Rosen, brought home the radicalness of quantum mechanics by showing how its failure to attribute determinate qualities to physical systems prior to measurement implies that spacelike-separated events are able to influence one another, in apparent violation of special relativity theory. These authors concluded from their result that quantum mechanics is an incomplete description of physical reality.

Attempts to provide a more complete description, while maintaining the empirical content of quantum mechanics, have centred around constructing so-called "hidden variable" theories. Unlike quantum mechanics these theories explain apparent action-at-a-distance effects by assigning "degrees of reality" to as yet unobserved properties of physical systems. The possibility of hidden variable theories which are empirically equivalent to quantum mechanics raises the prospect of underdetermination of theory by evidence. However, in 1964 John Bell [4] showed that, contrary what was first thought, (local) hidden variable theories do in fact make different predictions from quantum mechanics.[24]

Although Bell's analysis incorporates both quantum mechanics and hidden variable rivals, it does not involve a test-theory construction explicitly. Nevertheless, perhaps a test theory which incorporates all these theories is possible. Such a test theory would, presumably, improve our understanding the relationship between these theories and phenomena. It would also improve our classification of the competing theories, and thus clarify the relationship between quantum mechanics and its hidden variable counterparts, in a similar way that the PPN Formalism has done for competing theories of relativistic gravitation. Indeed, John Earman, claims that "various classification schemes" have already been devised by physicists working in the field.[25] Unfortunately, Earman provides no references.

My remarks in this section have been rather speculative, and I have not supported my tentative claims with explicit examples, as I have done in previous sections with regard to relativity theory. I consider the possible application of test-theory methodology to quantum theory as an important topic for further research. The orthodox view in physics is that quantum

[24]The results of experiments by Alain Aspect et al. [2], and also of more recent experiments, have favoured the predictions of quantum mechanics over those of its hidden variable rivals.

[25]Earman [19], p. 182.

theories of matter have been far better tested than Einstein's theory of gravity. However, the orthodoxy is not obviously correct if, as I suspect, quantum theories have only been tested hypothetico-deductively. For the test-theory method, which has been used in gravitational research, enables much stronger connections between theory and evidence to be established than does the hypothetico-deductive method. What is more, quantum theories, even compared to general relativity, have features which are highly counter-intuitive, some would say "unintelligible". Thus, it would be valuable to know, especially for the project of constructing a more unified theory of the fundamental interactions, whether, and to what extent, the various elements of our current quantum theories can be supported by test-theory-type arguments.

4.9 What test theories cannot do

One aim of my study of historical test theories in Chapters 2 and 3 was to show what test theories can do, and to show thereby how important to physics the test-theory method is. In the present chapter, however, I have explored not the achievements of this method but rather its limits. The lower or observation limit is, as I indicated in Section 4.1, easy to define. Defining the upper or theoretical limit of the method, is not not so easy, however, and for this reason I have, in the present chapter, explored this limit piecemeal by attending to specific conceptual issues in the foundations of physics.

Regarding the issue of distant simultaneity, I explained why recent test-theory arguments for determinate distant simultaneity relations fail. I also indicated why, more generally, a determination of these relations is at present beyond the reach of test theories. The reason seems to be that in the current conceptual context the parameter which quantifies distant simultaneity relations, ϵ, is not a test-theory parameter. It cannot be determined empirically. It can, however, be fixed to some desired value simply by choosing an appropriate coordinate system. The coordinate-dependent nature of ϵ, and the fact that ϵ cannot be excised from relativistic kinematics in the same way that flat spacetime can be excised from gravity theory, constitute the main stumbling blocks to solving the problem of distant simultaneity. Nevertheless, this problem is, I argued, a minor one, because distant simultaneity relations condition only slightly other elements of physical theory. Our interpretation of physical quantities is altered very little, and in many cases not at all, by changing the value of ϵ.

In contrast, changing our geometrical commitments has a profound effect on our interpretation of physical quantities. Fortunately, the problem of geometry is more tractable than the problem of distant simultaneity. In a relativistic context, test theories are able to secure for us determinate geometrical relations. The PPN Formalism, for example, contains an empirically determinable parameter (γ) which quantifies the extent to which a material source curves three-dimensional space. More generally, this test theory represents metric theories of gravity with different spacetime geometries. In some of these theories spacetime is curved and dynamical, in others it is curved and non-dynamical, and in still others it is flat and non-dynamical. The PPN Formalism enables us empirically to choose between these alternative theories of gravity. Equivalently, it allows us empirically to condition our beliefs about spacetime geometry. What test theories cannot do alone is determine for us whether or not gravity is metrical in character. Only by bringing less formal (though, in my view, eminently plausible) methodological principles to bear, are we able to reason our way from the results of test theories to the proposition that gravity is metrical.

Regarding causation, I showed how test theories, if anything, create rather than solve the dilemma of whether we should regard efficient or final cause conceptions as objective. I pointed out how in classical mechanics Hamilton's Principle of Stationary Action constitutes a mathematically sophisticated and powerful alternative to Newton's dynamical conceptions. I defended the view that both Newton's and Hamilton's conceptions, while abstract, nevertheless retain something of the causal character of previous, less sophisticated, dynamical conceptions. The possibility of test theories for both Newtonian forces and Hamiltonian actions makes it difficult, I argued, for us to decide whether efficient or final causes in mechanics are truly objective. Indeed, the close relationship which can be exhibited between efficient- and final-cause physical conceptions indicates that in physics the two sets of conceptions are not mutually exclusive. If anything, this relationship suggests that we look for a more formal conception of cause (based on symmetries, perhaps) which unites but transcends the categories of efficient and final cause.

Regarding quantum physics, I acknowledged the disparateness of Newtonian and quantum mechanical conceptions, but argued that already existing results suggest that Newtonian methods are not entirely alien to quantum physics. On the basis that test theories for classical actions are possible, I speculated that test theories might also be possible for quantum actions. On the basis that Einstein's argument for the quantum nature of radiation can be reconstructed as a Newtonian deduction from phenomena, and on the basis that there exist empirically inequivalent (hidden variable) rivals to standard quantum theory, I speculated that test theories for the quantum mechanical framework itself might be possible. Whether or not test-theory methodology applies in quantum physics is important if we are concerned about how strongly quantum theories can be empirically conditioned.

From the results of this chapter I draw the following conclusions. We can after all provide a general characterisation of the upper or theoretical limit of test-theory methodology. This limit is the point at which elements of theory no longer enter directly into the prediction and explanation of phenomena, but rather are constitutive of those elements which do so enter. By themselves, Newton's Laws of Motion and Hamilton's Principle of

Stationary Action do not predict or explain anything, but once forces and actions are specified predictive, explanatory theories are obtained. Correspondingly, it does not seem possible to provide a test theory in which are represented both Newtonian and Hamiltonian dynamical principles, yet test theories for specific Newtonian forces and specific Hamiltonian actions are certainly possible.

Of course, elements of theory which are at one time regarded as purely constitutive can later lose that status, as physics undergoes conceptual development. Prior to Einstein, geometrical relations were regarded as purely constitutive, non-empirical elements of theory. It is hardly surprising that test theories for geometry were not constructed then. But Einstein's work, by forging a connection between geometry and gravity, and thus revealing explicitly the empirical status of geometrical relations, made test theories for geometry possible. On this basis I think it would thus be reckless to conclude that elements of theory which today cannot be conditioned empirically by means of the test-theory method will remain forever immune from such empirical conditioning. Nevertheless, at any given time "test-theory determinability" does constitute a demarcation criterion for separating those elements of theory which are presently subject to empirical check from those which cannot have been developed in a way which is straightforwardly empirical.

One thing is sure. It is not possible for test-theory arguments to deliver concepts. Rather, test theories require concepts as constituents of a framework within which such arguments can be mounted. Test-theory methodology certainly shows how we can condition empirically many elements of our physical theories, yet for this conditioning to be possible some conceptual framework must first be given. How concepts and conceptual frameworks in physics are formed is the subject of Peter Harman's [37] book *Metaphysics and Natural Philosophy.* Harman argues that physical theories rest on metaphysical as well as on empirical constraints. He argues that physicists, motivated by a rationalist quest for intelligibility, have used metaphysical argument and conceptual analysis to develop physical concepts. One of Harman's examples is the development of the field concept in nineteenth century physics. Philosophically, Harman contends, this development was motivated by what Newton's successors regarded as the unintelligibility of Newtonian atomism and its concomitant notion of action-at-a-distance.[26]

I think Harman makes a good case for the view that the philosophical search by physicists for intelligible physical conceptions has led to fruitful theoretical development in physics. Harman's discussion shows what is wrong with the operational view of science, according to which concept formation involves little more than scientists agreeing on their definitions before getting on with the important empirical work. Of course, Harman's views also seem to conflict with my own understanding of Newton and Einstein. For I claimed in Chapters 2 and 3 that an important philosophical precondi-

[26]Harman [37], p. 81ff.

tion for the creative work of these two physicists was that they relinquished the quest for intelligibility in favour of establishing strong connections between theory and evidence. In so doing, both these physicists were led to embrace physical theories which many of their contemporaries found quite unintelligible.

Yet there is room, I think, for a healthy dialectic between rationalist and empiricist tendencies in physical thought. Indeed, my view is that such dialectic is necessary for physical thought to be healthy. Without the products of rational speculation there would be available no conceptual frameworks for the construction of test theories. Without test theories there would be no strong empirical check on the products of rational speculation.

Thus, we can accept, as did Newton, that Descartes and other seventeenth century mechanical philosophers were right to reject the mysterious occult quantities of Aristotle and seek more intelligible explanations for natural phenomena. Yet we can also endorse Newton's tacitly held view that this search went overboard in rejecting all but push-contact notions of force. With the help of his test theory, Newton showed that a more abstract conception of mechanism could provide explanations that traditional mechanical hypotheses had no hope of providing. We can accept that scientists after Newton were right to question the intelligibility of Newtonian atomism and action-at-a-distance, for this led ultimately to the development of field theory. Yet we can also agree with Einstein that nineteenth century physicists went too far in seeking some kind of mechanical reduction of fields. By means of his Newtonian-styled derivation of the Lorentz transformation, Einstein showed that a more abstract conception of the demand for locality could explain phenomena that the neo-Cartesian ether had no hope of explaining.

Harman's thesis, that metaphysical argument has played a crucial role in the development of physics, clarifies the nature of the conceptual basis of test theories. His study shows what kind of methods must be applied to elements of theory which lie beyond the upper or theoretical limit of test-theory methodology. His study shows, in particular, how rationalist, or philosophical, methods make the methods of empirical science possible. I will return to this discussion in Section 6.2, where I consider the possibility of test theories being used to further unify the fundamental physical interactions.

CHAPTER 5

Test-theory methodology and the philosophy of science

In the present chapter I examine some influential accounts of confirmation from the philosophy of science. I show how test-theory methodology sheds new light on these accounts, but also how these accounts instruct us further as to the character and function of test theories. Among the philosophical accounts of confirmation I discuss are logical-positivist accounts, post-positivist "historicist" accounts, as well as some neo-positivist accounts based on "bootstrapping" and on "demonstrative" and "eliminative" induction. I do not aim to provide a comprehensive analysis and critique of the various accounts of confirmation I discuss. Rather, I seek to highlight just those philosophical doctrines which significantly illuminate test-theory methodology. It is no accident that logical positivism, in both its older and more contemporary guises, constitutes the main subject of this chapter. The formal character of the test-theory method makes it from the start more conformable to positivist than to historicist accounts of confirmation. I will argue that test-theory methodology shows how the reductionist account, in particular, makes sense of the practice of physics to a far greater extent than is generally acknowledged by philosophers today.

In Section 5.1 I reassess the reductionist account of confirmation (and meaning) in light of how well test-theory methodology conforms to it. In Section 5.2 I discuss the degree to which physical theory is speculative in character, by comparing test-theory methodology with the hypothetico-deductive account of confirmation. In Sections 5.3 and 5.4 I argue that test-theory methodology undermines the philosophical doctrines of meaning holism and conventionalism. In Section 5.5 I use test-theory methodology to make sense of Karl Popper's view that experiments can be used definitively to rule out, or falsify, scientific theories. In Section 5.6 I argue that test-theory methodology undermines Thomas Kuhn's conception of 'normal science' and his doctrine of incommensurability. In Section 5.7 I discuss how rival physical theories compete with one another, by comparing test-theory methodology with Imre Lakatos's Methodology of Scientific Research Programmes. In Section 5.8 I clarify the similarities and differences between test-theory methodology and the closely-related "bootstrap" account of confirmation due to Clark Glymour. In Section 5.9 I evaluate John Earman's thesis that test-theory arguments for relativistic gravity (and by implication all test-theory arguments) are best characterised as eliminative inductions. In Section 5.10 I discuss the warrant for test-theory methodology, and I examine the bearing of this issue on the doctrine that nature is systematic, or unified.

5.1 What's right with reductionism

Philosophical theories of confirmation tell us how scientific theories are (or ought to be) conditioned empirically. Clark Glymour explains how modern theories of confirmation owe their existence to the logical positivists' search for a criterion of empirical significance to distinguish ordinary and scientific discourse from metaphysics and nonsense.[1] The positivists believed that for a statement to be meaningful it had to be empirically confirmable in principle. This belief eventually led them to investigate the specific nature of the confirmation relation. I discuss, in Sections 5.1–5.4, two quite different positivist accounts of confirmation, namely, the reductionist and hypothetico-deductive accounts. As is well known, the demise of the reductionist account, and the rise of the hypothetico-deductive account, has made more plausible a radically holist view of confirmation and meaning, and also a radically conventionalist view of scientific theory. So far as physics is concerned, however, I argue that the reductionist account more closely conforms to the practice of physics, and to test-theory methodology in particular, than does the hypothetico-deductive account. On this basis, I conclude that, so far as theoretical physics is concerned, the doctrines of holism and conventionalism are largely false.

According to an early, but untenable, account of confirmation, which the positivists soon abandoned, theoretical propositions are confirmed by deducing them from a finite set of observation statements.[2] Since, however,observation statements typically are singular statements, while many theoretical propositions are universal in character, this account renders unconfirmable much of what is normally regarded as highly confirmed scientific theory. Since this account of confirmation obviously is unsatisfactory, we are left with only two possibilities. Observations can confirm a theoretical proposition either by implying instances of the proposition or by being implied by the proposition. Reductionist accounts of confirmation fall into the first category, while hypothetico-deductivism falls into the second.

For positivists, reductionist accounts of theory-evidence relations presuppose reductionist criteria of empirical significance. According to such criteria theoretical terms (and hence theoretical propositions composed of such terms) acquire their significance, or meaning, by being reducible to, or definable by means of, observation terms. One influential, but rather inchoate,

[1] Glymour [31], pp. 11–12, pp. 29–30.

[2] Hempel [39], p. 104, gives the meaning criterion associated with this account.

version of reductionism was Percy Bridgman's operationalism, according to which theoretical terms are "synonymous" with specific physical operations (or sets of operations) involved in making observations and measurements.[3] More sophisticated versions were advocated by Rudolph Carnap [8] and Hans Reichenbach [66]. These philosophers maintained that theoretical terms should be reducible to observational terms by means of conventionally stipulated "correspondence rules" or "coordinative definitions", which contain both theoretical and observational terms. In contrast to Bridgman's vague idea of a correspondence between theoretical and observational terms, Carnap and Reichenbach proposed that this correspondence be made explicit and systematic by means of analytic rules and definitions.

In seeking to make clear and precise the empirical meaning of each theoretical term, reductionists like Carnap and Reichenbach sought a relevance relationship between theory and evidence. They did not believe that a given phenomenon could confirm just any piece of a theory, or all of it. Rather, they believed that a phenomenon could confirm, and thus be relevant to, just some specific piece. I have provided ample evidence, in Chapters 2 and 3, that test-theory methodology respects this intuitive idea of evidential relevance. Newton's test theory shows, for example, how the observed quiescence of planetary aphelia is relevant to the distance-dependence of the Sun-centred force acting on the planets. The PPN Formalism shows how the phenomenon of light bending by the Sun allows us to determine the extent to which matter curves three-dimensional space. More generally, the parametric structures of test theories provide explicit and systematic correspondence between pieces of theory and pieces of evidence, and to this extent test-theory methodology clearly realises the reductionist aspirations of Carnap and Reichenbach.

Of course in test-theory methodology there is no term-by-term reduction of theory to observation by means of analytic correspondence rules. The correspondence established by test theories is made possible only by prior assumption of other *substantial* elements of theory. These elements constitute the conceptual presuppositions, and the defining and auxiliary assumptions, of our test theories. I need hardly assure the reader that these assumptions are not analytic truths. Propositions describing the shape of planetary orbits, for example, or the centripetality of a force, are clearly synthetic, rather than analytic, in character.

Because test-theory methodology requires substantial (i.e synthetic) background assumptions to establish relations between pieces of theory and pieces of evidence, these relations may be regarded as less direct, and therefore weaker, than the relations reductionists hoped to establish. In one very important sense, however, this weakness is a strength. For, it enables test theories not merely to determine, but to overdetermine, elements of theory empirically. The PPN Formalism allows, for example, both the phenomenon of light bending and the transit time of radar signals to inner planets inde-

[3] Bridgman [7], p. 5ff.

pendently to determine the value of the parameter quantifying the extent to which matter curves three-dimensional space. Evidently, these phenomena are rather different in kind, and the operations, or observational procedures, by which they are measured are different also. Yet, the PPN Formalism brings these disparate phenomena to bear on, and thus be relevant to, precisely the same element of spacetime theory.

In contrast, then, to the very direct, one-to-one relationship between evidence and theory envisaged by reductionists, test-theory methodology realises a slightly less direct, but many-to-one, relationship. For this reason, test-theory methodology makes possible a confirmation relation which is, in a significant way, stronger than the relation proposed by reductionism. Historically, it was the very directness of the reductionist confirmation relation which proved the undoing of reductionist philosophy. Carnap and Reichenbach were simply unable to find the analytic correspondence rules they needed to establish the reduction relation.

The untenability of historical reductionism can be understood, from the perspective of test-theory methodology, in the following way. A test-theory construction would provide the kind of direct connection between theory and evidence that reductionists seek only by having all of its elements parameterised in test-theory fashion. In such a state all substantial features of the test theory would be open to empirical determination simultaneously, none would be assumed. Now we know, from the results in Chapter 4, that it is just not possible to parameterise, in test-theory fashion, some of the very basic assumptions of our test theories, given our current concepts. But suppose that this were possible. Even then, I believe, the idea of a test theory with all elements parameterised simply cannot be made coherent. For, by parameterising an element of some physical law we make that law's form indeterminate to a small degree. But a law with every one of its elements parameterised would constitute a test theory altogether without form. It would lack any kind of structure by which to interrelate its parameterised elements. The absurdity of this idea indicates, I think, the unfeasibility of the reductionists' demand for a very direct connection between theory and evidence. It also indicates a limiting feature of test-theory methodology: reliance on substantial, determinate theoretical background assumptions is unavoidable.

The positivists' insistence on formal clarity led them to demand a very direct, but ultimately unworkable, connection between pieces of theory and pieces of evidence. Test-theory methodology constitutes a feasible corrective to this overly strong and unworkable demand, without abandoning the intuitive idea of evidential relevance, and thus without abandoning the positivist hope that the empirical meaning of individual pieces of theory might be made formally clear. Nevertheless, that test-theory methodology is able to establish formal, systematic connections between pieces of physical theory and pieces of evidence indicates that there is a lot more right with reductionism than many philosophers suppose.

In correcting the reductionist view, test-theory methodology also shows why we should be careful not to overcorrect it. Consider the anti-reductionist view according to which it is not possible to understand our spacetime commitments by looking to this or that body in motion, but only by looking to our general dynamical principles of motion. According to this view, what commitments we have to spacetime structure cannot be determined by analysing measurements but only by analysing the conceptual presuppositions of our physical laws. John Earman [18], for one, seems to support something like this view in his book *World Enough and Space-Time*. Earman counsels (in Section 3.4) that we need to look to the dynamical symmetries of our physical laws to determine what our spacetime commitments in fact are.[4] However, modern test theories, like Robertson's and the PPN Formalism, show us that it is, after all, possible to establish a more direct connection between phenomena and various of our spacetime commitments than this anti-reductionist view would have us believe.

5.2 The hypothetical character of physical theories

Reductionist programmes failed to make sense of the idea that phenomena may support a theoretical proposition by implying instances of the proposition. Consequently, many positivists turned to the other possibility, that phenomena may support a theoretical proposition by being implied by the proposition. This constitutes the essence of the hypothetico-deductive account of confirmation. In addition to preserving the positivists' expectation that theory-evidence relations should be capturable in formal, deductive terms, many philosophers believed that the hypothetico-deductive account, in contrast to the reductionist account, conformed well to much of the everyday practice of science.

For positivists, a hypothetico-deductive account of confirmation presupposes a hypothetico-deductive criterion of empirical significance. According to this criterion theoretical propositions acquire their significance, or meaning, by having some observation claim or other deduced from them. According to the hypothetico-deductive method, scientific theories are first advanced speculatively, and then tested by comparing their empirical consequences with observations. Now there is no doubt that scientists, generally, spend much of their time testing theories in the hypothetico-deductive manner. However, according to the philosophical doctrine of hypothetico-deductivism, this method is the only way scientific theories may be tested empirically. Because test theories are widely used in physics, and because the hypothetico-deductive and test-theory methods differ significantly in the way they confirm theory, I cannot support the doctrine of hypothetico-deductivism.

[4]Earman's later study of the PPN Formalism (see Section 5.9) implies a change of viewpoint, though Earman does not bring the results of this study to bear on his earlier remarks about spacetime.

The hypothetico-deductive and test-theory methods are of course alike in some ways. Both methods are formal and deductive in character. They both (Popper's brand of hypothetico-deductivism excepted) construe the relationship between theory and evidence as a positive, confirming relation. By their lights, moreover, a direct, analytical connection between theory and evidence, such as the reductionists sought, is not possible. Rather, confirmation requires additional beliefs of a substantial, rather than merely definitional, character to mediate the relation between theory and evidence.

There are at least two ways, however, in which the hypothetico-deductive method is strongly distinguished from the test-theory method. Firstly, the test-theory method is more systematic than the hypothetico-deductive method. It involves empirically selecting a physical theory from out of a parameterised class of alternatives, rather than testing theories in piecemeal fashion. Secondly, our two methods support inferences which flow in opposite directions. So far as direction of inference is concerned, test-theory methodology agrees with reductionist methodology but is distinguished from hypothetico-deductivism.

The possibility of deriving theory from phenomena, via test theories, has important bearing not only on our understanding of theory confirmation but also on our understanding of theory discovery. In Section 2.8 I explained how hypothetico-deductivists tended to distinguish between the discovery of a scientific theory and its justification, because of their belief that theories must first be advanced speculatively, or hypothesised, before they can be confirmed empirically. I also pointed out that this distinction is somewhat counter-intuitive, because 'discovery' connotes success, and its seems presumptuous to describe a scientific theory as successful before it has undergone any kind of empirical testing.

However, test-theory methodology indicates that we need not subscribe to the counter-intuitive notion that a theory's discovery must occur prior to its justification. For, test-theory methodology does not require that we first conjecture a definite physical theory (i.e. a theory one can use to make predictions) before we can proceed to the testing stage. On the contrary, via test-theory constructions one can discover a physical theory precisely by deriving it, for the first time, from phenomena, thereby also confirming it. Indeed, it was primarily on the basis of his non-speculative test-theory argument that Newton asserted his priority in the discovery of the inverse-square law.[5] Thus, test-theory methodology, in contrast to hypothetico-deductivism, provides not only an account of theory confirmation, but also an account of theory discovery. This account might even be said to constitute a logic of discovery, for the reason that test-theory arguments establish formal deductive relations between theory and phenomena.

Because test theories are able empirically to determine many elements of physical theory, physical theory is in fact far less hypothetical, or speculative, than many philosophers today would be willing to acknowledge. Of course,

[5] See Section 2.8.

even the test-theory method involves a "hypothetical" element. As I remarked at the end of Section 4.9, the conceptual presuppositions of test theories typically include elements of theory which have been developed not by straightforward empirical means but by conceptual analysis and metaphysical argument. Because it is "hypothetical", hypothetico-deductivism, in contrast to test-theory methodology, makes explicit this necessary, but non-empirical, facet of scientific inquiry. In this way, hypothetico-deductivism draws our attention to an important, but implicit, feature of test-theory methodology. Nevertheless, the possibility of test theories shows that the speculative character of physical theory is in fact much more confined to certain highly constitutive elements of theory than the hypothetico-deductive account leads us to believe.

5.3 What's wrong with meaning holism

Hypothetico-deductivism makes plausible the doctrine of meaning holism. According to this doctrine, individual beliefs do not possess empirical meaning. Only systems or networks of beliefs can aquire significance through relationship to the phenomena. Meaning holism arises primarily from the weak character of the hypothetico-deductive confirmation relation. It arises, that is, from that fact that theory is confirmed hypothetico-deductively by implying phenomena, rather than being implied by phenomena.

To see this, consider a typical example of hypothetico-deductive confirmation. Newton's law of gravity is used to predict the relationship between Jupiter's orbital period and its mean distance from the Sun. To perform the required derivation one needs, in addition to the law of gravity, Newton's Second Law of Motion and some auxiliary assumptions specifying, for example, the shape of Jupiter's orbit and the perturbative effects of other planets. Suppose that the period/distance relationship thus derived agrees with observations. Which premise in the deductive argument should we regard as confirmed by these observations? It is natural to regard as confirmed the premise containing Newton's law of gravity. But are we compelled to do so? All of the premises are necessary for the argument to be valid, so why single out any one premise as special? Similarly, which premise shall we blame if observations contradict our prediction? The hypothetico-deductive method seems to give us no formal guidelines about how to distribute praise or blame among the various beliefs required for a hypothetico-deductive argument. This method fails, in other words, to establish a relevance relation between theory and evidence.

Attempts to remedy this defect, have concentrated on finding suitable formal constraints to "localise" confirmation in a hypothetico-deductive argument to particular premises. Glymour has reviewed these attempts and concluded that none of them are satisfactory.[6] This result suggests that individual theoretical propositions cannot be tested hypothetico-deductively,

[6] Glymour [31], pp. 32–47.

and that only sets of propositions may be confirmed or disconfirmed in this way. Thus, it does not seem possible, by the lights of hypothetico-deductivism, to specify the meaning of individual theoretical propositions in empirical terms. The result is a holist criterion of empirical significance, according to which only complex networks of beliefs can acquire significance through relationship to phenomena. Individual beliefs, by contrast, are quite without meaning, at least in any empirical sense. In the words of one philosopher, "our statements about the external world face the tribunal of sense experience not individually but only as a corporate body".[7]

The methodological basis of meaning holism is the doctrine of hypothetico-deductivism. I have already rejected this doctrine, however, for the reason that test-theory methodology provides an alternative (and in my view far better) way of confirming scientific theories empirically than is provided by hypothetico-deductive methodology. Indeed, test theories show explicitly how specific phenomena may be used to condition specific elements of theory, thereby clarifying the empirical meaning of those elements, and undermining the holist's claim that individual beliefs, or parts of theory, are entirely without empirical meaning.

If there is anything redeeming about the doctrine of meaning holism it is that it reminds us that even test-theory arguments are always made in light of some theory or other. It is just not possible to establish a theory-independent connection between individual theoretical propositions and phenomena. Hypothetico-deductivism and test-theory methodology share the assumption that theory-evidence relations are reticulate character, and in so doing they indicate that the meaning of individual beliefs, or parts of theories, is a not entirely a theory-independent matter.

Given the failure of hypothetico-deductive arguments to establish relevance relations, we might ask why, in the practice of science, such arguments are often regarded as confirmatory of individual theoretical propositions, for example, of individual theoretical laws. Clark Glymour suggests that this often occurs because the relevance of theory to evidence has, in some way or other, already been established, and hypothetico-deductive arguments then inherit this bearing. For example, Newton established the relevance of celestial motions to gravity theory in a non-hypothetico-deductive manner, and all subsequent gravity theories have been confirmed or disconfirmed on the basis of their predictions of such motions. Glymour suggests further that we should not expect arguments establishing relevance to appear ubiquitously in science, but to expect them most commonly with the development of new theories, the apparent failure of existing ones, and "when fine-grained questions arise as to the relative importance of various experiments and observations".[8]

Modern test theories for relativity certainly bear out these remarks of Glymour's. Given the current proliferation of relativistic theories of gravity,

[7] Quine [64], p. 41.
[8] Glymour [31], pp. 169–172.

physicists want to know what elements these theories have in common, and how well incompatible elements are tested relative to one another. Test theories help answer these questions. Given the contemporary drive for unification of the fundamental interactions, which will require theory change, physicists also want to know what parts of our spacetime theories are well-supported, so that they may concentrate on analysing and testing parts which may be of more dubious standing. Modern test theories make such knowledge possible by establishing the relevance relations required.

5.4 The overdetermination of theory by evidence

Conventionalism is the doctrine that scientific beliefs (theoretical beliefs especially) are not determined uniquely by phenomena but possess a certain amount of arbitrariness. According to conventionalists, this arbitrariness must be removed by conventional stipulation before it becomes possible to state scientific laws which may be confirmed empirically. In the present section I critically discuss, in connection with test-theory methodology, the two forms of conventionalism which arise from positivist accounts of confirmation (and meaning).

The first form of conventionalism arises from reductionism. Carnap and Reichenbach imagined the reduction of theory to observation to be carried out by means of "correspondence rules" or "coordinative definitions", which were to contain both theoretical and observational terms. The labels 'rule' and 'definition' are apt. These labels indicate explicitly that feature of reduction sentences which was to distinguish them from other sentences containing either purely theoretical or purely observational terms: their analytic character. The truth of reduction sentences was to be demonstrable not by reference to matters of fact, but merely by defining their constituent terms in an appropriate way. Specifically, the theoretical terms in reduction sentences were to be defined so that they acted as convenient abbreviations for statements containing observational terms.

Nevertheless, Reichenbach and other reductionists believed that once the precise character of their analytic reduction sentences had been conventionally stipulated then all other theoretical beliefs would be empirically conditionable. In particular, physical laws containing temporal and spatial variables would be so conditionable after reduction sentences specifying the structure of time and space had been stipulated.

Test-theory methodology does not support this form of conventionalism, however. For test theories enable phenomena to determine not only elements of theory which reductionists regard as synthetic, but also elements they regard as analytic, and therefore conventional. For example, the PPN Formalism enables phenomena to determine theoretical elements which specify the geometries of space and spacetime. Far from supporting the reductionist belief that the deepest elements of physical theory are underdetermined by the evidence, test-theory methodology actually makes possible the empirical overdetermination of such elements.

In a similar way, test-theory methodology undermines that other form of conventionlism which is associated with hypothetico-deductivism and the doctrine of meaning holism. According to this doctrine, the locus of meaning is not the individual belief, but rather complex networks of beliefs. An immediate consequence of meaning holism is the breakdown of the analytic-synthetic distinction which was so crucial to the reductionist programme. For, the categories of analyticity and syntheticity are categories of meaning. But if individual beliefs are without meaning then it is not possible to apply these categories to them. Now analytic beliefs are those beliefs which are true or false irrespective of experience, whereas synthetic beliefs are true or false only by virtue of experience. But what sort of beliefs are they that are neither analytic nor synthetic? That is, what sort of beliefs are they that can either be retained or abandoned come what may in experience? They can only be conventions, which, by their very nature, are underdetermined by all possible experience.

According to Willard Quine, a holist, "the total field [of our beliefs] is so underdetermined by its boundary conditions, experience, that there is much latitude of choice as to what statements to reevaluate in the light of a single contrary experience".[9] Thus, it is a short step from meaning holism to full-blown conventionalism. It is clear that this form of conventionalism is different than the form arising from reductionism, because now all our beliefs are taken to possess arbitrariness, not just a privileged subset of analytic beliefs.

However, to the extent that test-theory methodology establishes relevance relations between theory and evidence, test-theory methodology undermines holism, and thereby undermines that form of conventionalism which is based on holism. Indeed, we have already seen how test-theory methodology enables quite disparate phenomena identically to determine, and thus to overdetermine, even highly constitutive elements of physical theories. What strength conventionalism may gain from being associated with the widespread application of the hypothetico-deductive method is diminished considerably by the fact that test-theory methodology is possible.

5.5 How crucial falsifying experiments are possible

Karl Popper recognised the weakness of the hypothetico-deductive confirmation relation. He did not simply accept this weakness and acquiesce in favour of holism and conventionalism. Instead, he advanced his own critical brand of hypothetico-deductivism, falsificationism, which, he claimed, did not suffer the weakness suffered by standard hypothetico-deductivism. According to falsificationism, scientific theories cannot be confirmed, but only falsified, by experience. Popper was no positivist, of course. He did not

[9] Quine [64], pp. 42–43.

regard his method of falsification as supportive of a falsificationist criterion of empirical significance, but rather as providing a demarcation criterion for distinguishing science from non-science.

Popper motivated his falsificationist methodology by appeal to logic, and by appeal to the rationality and history of science. Popper accepted Hume's critique of inductive reasoning, but also believed that of all human activites the practice of science was the preeminently rational one.[10] Popper worried that the positivist's version of hypothetico-deductivism could not make sense of the rationality of science, because it seemed to contain an inductive, and therefore irrational, element. In addition, Popper regarded the positivist's preoccupation with the logical analysis of theories, as insufficiently critical to account for how scientific theories in fact change and develop.[11]

Popper believed that he was able to make sense of both the rationality of science and the growth of scientific knowledge by reconstruing the hypothetico-deductivist confirmation relation as a critical, falsifying relation. Traditional hypothetico-deductive arguments are in fact inductive, because they employ singular statements (the predictions deduced from a theory) to support universal statements (the laws or hypotheses of a theory). In Popper's scheme, however, hypotheses are not confirmed by correct predictions, but only falsified by incorrect ones. Thus, falsificationist methodology retains the formal, deductive aspects of positivist accounts of confirmation, yet avoids the Humean conclusion that science is without rational basis by denying that induction is ever involved in the conditioning of scientific theories. What is more, the critical spirit of falsificationist methodology makes sense of our intuitive idea that science is not merely an empirical but also a critical, non-doctrinaire enterprise, prepared to overturn its theories in light of new experience.

There are two very good reasons why falsificationists should look favourably upon the test-theory method. The first reason is that this method establishes relations between theory and evidence which are purely deductive in character. By this method elements of theory are, in Newton's words, "deduced from phenomena". It is true, of course, that practitioners of test-theory methodology typically use the products of test-theory arguments to warrant inductively general theoretical laws. (Newton and Robertson, do this, for example. Indeed, they make explicit the inductive aspects of their reasoning.) Nevertheless, falsificationists cannot deny that test-theory methodology is able to provide strong empirical constraints on the form of what they call scientific conjectures.

The second reason why falsificationists should look favourably upon test-theory methodology is that it allows them to make sense of their idea that crucial falsifying experiments are possible. Falsificationists have found it difficult to make sense of this idea, because their account of theory-evidence

[10]Popper [62], p. 27ff.

[11]Popper [62], pp. 49–50.

relations remains essentially a hypothetico-deductive account. Hypothetico-deductivism, in whatever form, provides no formal guidelines as to where we should direct the arrow of modus tollens. Does contrary evidence falsify the laws of our scientific theory, or merely the auxiliary assumptions to those laws? In a hypothetico-deductive context there is no avoiding the conclusion that scientific theories are not strongly falsifiable.

This not the case, however, in the context of test-theory methodology. If two or more different phenomena overdetermine the value of a test-theory parameter, then they provide a very strong reason for rejecting alternative values of that parameter, and thus rejecting physical theories associated with those alternative values. The reason test-theory methodology is able to do this is because test-theory methodology, unlike falsificationism, is able to establish relevance relations between theory and evidence, and is thus able to pinpoint which element of theory is impugned by contrary evidence. Test-theory methodology thereby supports the falsificationist's belief that crucial falsifying experiments are possible, a belief which is impossible to justify in a hypothetico-deductive context. It seems to me that falsificationists should, in light of this fact, abandon hypothetico-deductivism in favour of test-theory methodology (in so far as physics is concerned, anyway).

For my part, I am no falsificationist. I believe that inductive arguments can, and do, provide positive warrant for theories. Practitioners of test-theory methodology also believe this. Newton, Eddington, Robertson, Nordvedt, Will, Mansouri, and Sexl all concur that test-theory arguments confirm those physical laws whose instances constitute the conclusions of test-theory arguments. Not one of these physicists adopt a falsificationist line with respect to the test-theory method. In my view, test-theory methodology represents the best of both positivist and falsificationist accounts of theory-evidence relations. For it shows how phenomena may emphatically support a physical theory while at the same time definitively ruling out, or falsifying, its rivals. In this way test-theory methodology supports the critical spirit of falsificationism, while avoiding its sceptical overtones.

A discussion of falsificationism seems as good a place as any to examine the case in which test theories themselves conflict with the evidence. This kind of conflict occurs when phenomena yield differing values of a given test-theory parameter. In such cases there is clearly something wrong with either the conceptual presuppositions or with the defining and auxiliary assumptions of our test theory. But which assumption in particular is at fault? We have to admit that there is no formal procedure for deciding here. This fact represents a further limiting feature of the test-theory method—a feature which arises from the ineradicable hypothetical character of our test-theories' conceptual presuppositions. In degree, this limiting feature is less severe in the context of test-theory methodology than it is in the context of hypothetico-deductivism, because in the former context physical theories contain less hypothetical content to begin with than they do in the latter context. Test theories simply are less liable to be disturbed by novel phenomena than are the joint premises of a hypothetico-deductive argument. Nevertheless, because the extent to which relevance relations can be clearly established by test-methodology is limited, less formal procedures will usually be needed to decide which test-theory assumptions are impugned by contrary evidence.

5.6 Normal science, theory change, and commensurability

Popper's critical methodology was motivated not only by worries about induction, but also by a desire to come to terms with the growth of scientific knowledge, and with the phenomenon of theory change in particular. While Popper sought a more historically informed account of science than the positivists had provided, his falsificationist methodology was nonetheless strikingly formalist in its technical details. Popper has in fact been roundly criticised for not having paid enough attention to the actual history of science. A more thoroughgoing historical approach to methodology, one which represents a more radical departure from positivist views, and one which is ostensibly more sensitive to historical fact than either positivism or falsificationism, was advanced by the historian of science Thomas Kuhn [43].

Kuhn's approach is a coarse-grained one. It concerns not so much the minutiae of scientific practice as it concerns broad historical patterns of scientific development.[12] In particular, Kuhn's approach does not concern formal relations that scientists might establish between theory and evidence.[13]

According to Kuhn, science develops in a cyclical fashion. There are extended periods of conceptual stability (normal science), which are punctuated by much shorter periods of conceptual upheaval (scientific revolutions), during which scientists defect from one set theoretical conceptions (paradigm) to another. A typical period of normal science is dominated by the concerted attempts of scientists to elaborate a paradigm, to use it, that is, to explain phenomena and to further technological development. Sporadic problems which scientists fail to solve (anomalies) do not impugn the paradigm, but merely the ingenuity of the scientists in applying it. When very many anomalies accumulate, however, confidence in the paradigm is undermined. A period of uncertainty (crisis) follows, during which rival conceptual schemes are formulated. A revolution occurs when a majority of scientists abandon the old paradigm in favour of one of its rivals.

While the number of anomalies at any given time constitutes a gauge of

[12]This is true, at least, of the most interesting and plausible versions of Kuhn's view. I will pay no heed in this section to unworkable variants, which accord a high degree of "slippage" to Kuhn's basic concepts of 'paradigm', 'crisis' and 'revolution' in an attempt to apply them to smaller scales, even to the day-to-day workings of science.

[13]Neither does it exclude these relations, however. In support of Kuhn, and against Popper, Hilary Putnam [63] states that "practice is primary", that "ideas are not just ends in themselves", that "[t]he primary importance of ideas is that they guide practice, that they structure whole forms of life" (pp. 237–238). Nevertheless, Putnam makes sense of Kuhn's historicist account of theory-evidence relations in formal, deductivist terms.

how well a paradigm is doing, it is Kuhn's view that the establishment and eventual overthrow of a paradigm is influenced much less by rational and empirical considerations than by social and political factors. According to Kuhn, for example, young scientists are "indoctrinated" into a paradigm.[14] This paradigm is carefully protected from revision by attributing amomalies to failures in implimenting the theory rather than to any weakness in the theory itself. This same paradigm is eventually abandoned, however, upon the outcome of a debate which is more polemical than rational in character. The reason, according to Kuhn, is that method is so theory-dependent, that there exist no criteria by which rationally to compare and evaluate rival theories. By Kuhn's lights rival scientific theories are so disparate they are "incommensurable". A "gestalt switch" is thus required if scientists are to abandon one theory in favour of another.

Clearly, Kuhn's construal of the historical development of science supports a historical, as opposed to a formal, characterisation of theory-evidence relations. Accordingly, Kuhn's account highlights historical, as opposed to formal, dimensions of theoretical meaning. Kuhn allows that explanatory relations may be established between theory and evidence, and he does not rule out the possibility that such relations may be of a formal character. What Kuhn believes, however, is that the ways in which these relations are established, are so peculiar to their associated conceptual scheme that there can be no theory-independent characterisation of these ways, such as Popper and the positivists attempted to give. Consequently, the issue of empirical meaning, if it arises at all with Kuhn, is a radically theory-dependent issue.

It is important to realise that Kuhn's conception of 'normal science' is plausible only if Popper is wrong about the falsifiability of theories. For if theories were strongly falsifiable, this would undermine Kuhn's view that scientists are able to protect their theories from revision despite contrary evidence. But if theories could not be protected from revision, then Kuhn would have less motivation for thinking that one theory, or paradigm, could come to dominate scientific inquiry over an extended period. Now test theories not only enable physical theories to be definitively ruled out by evidence, and thus strongly falsified, they also (typically) represent a class of rival theories within their structures. Hence, test-theory methodology shows how it is possible to keep a number of rival theories in view without necessarily having a strong commitment to any one theory.[15]

Yet, we have good reason for distinguishing, say, between the Newtonian and post-Newtonian eras in physics. Kuhn is surely right to claim that there exist periods of conceptual stability in the history of physics. We can

[14]Kuhn's contention that science texts "indoctrinate" students into the current paradigm, to the exclusion of all rival conceptions, is not borne out by the physics texts of Will [87] and Misner et al. [54]. For these texts treat of test-theory constructions, which keep in view physical theories which are rival to the most widely accepted theory.

[15]Kuhn does allow for the simultaneous existence of rival theories, but only in the pre-scientific and revolutionary phases of scientific development. Modern test theories, in contrast, have been developed in what Kuhn regards as a period of normal science.

make sense of this fact by recognising that test theories are capable not only of falsifying physical theories but also of confirming them. Empirical determinations of test-theory parameters tend to converge towards a single theory, or towards a class of theories which is tiny relative to the class of all theories represented by the test-theory framework.

What is more, test theories enable physicists empirically to overdetermine elements of theory. Empirical overdetermination provides good reason for believing such elements to be robust, and therefore reliable for explaining phenomena. While it is true that physicists can, and do, attempt to explain phenomena with theories other than the one (or few) upon which test-theory arguments are converging, these attempts are relatively infrequent because test theories provide strong reason for believing such theories to be false. Hence, despite the fact that test theories usually represent an infinite number of rival physical theories, the converging and overdetermining character of test-theory arguments is in accordance with Kuhn's view that particular theories come to dominate the practice of science.

Test-theory methodology departs most strongly from Kuhn's viewpoint in that it provides a way of uniformly evaluating rival physical theories, a fact which seems to undermine Kuhn's doctrine of incommensurability and its concomitant notion of the radical theory dependence of method. The application of test theories in both classical and post-classical physics constitutes an element of practice which unifies methodologically these two conceptually distinct periods of scientific inquiry. Even more remarkable, though, is the possibility that test theories can represent physical theories from two such distinct periods in the history of science. The test theory in Section 3.4, for example, incorporates both Einsteinian and Galilean kinematics. Test theories such as this seem to imply that Kuhn is simply wrong to believe that there is no way, methodologically speaking, to transcend rival theories and judge them independently on evidential grounds.

In fact there are two possibilities. Either Kuhn's doctrine of incommensurability is out-and-out false, or Kuhn is wrong to characterise the change from Newtonian to Einsteinian physics as a scientific revolution (in his sense of the term). My view is that Kuhn's doctrine is not out-and-out false. Successful theories will always differ conceptually in some way from their predecessors, and occasionally this difference will be quite marked. The change from Descartes' mechanistic view of motion to Newton's understanding of motion in terms of abstract forces involves sufficiently great changes in both concept and method that Kuhn's doctrine of incommensurability largely applies. There is no suggestion that we could ever derive Cartesian from Newtonian mechanics. Likewise, there is no possibility that we could construct a test theory which would incorporate both these theories. In contrast, Newtonian and Einsteinian mechanics are supremely commensurable.

My view, then, is that while Kuhn's doctrine of incommensurability is not out-and-out false, test-theory methodology indicates that this doctrine should be weakened to allow for degrees of incommensurability. Kuhn's claim that a "gestalt switch" is required for genuine theory change to occur will, correspondingly, be more accurate of some episodes in the history of science than in others. Test-theory methodology thus challenges Kuhn's view that psychological and socio-political factors will necessarily be dominant in effecting a change of allegiance from one theory, or paradigm, to another. If Kuhn's doctrines apply at all to physics, they will apply most clearly to the level at which test-theory methodology is no longer able to establish clear and systematic relations of relevance between theory and evidence. They will apply, that is, to the level at which physical theory is no longer strongly falsifiable.

5.7 How physical theories compete with one another

Like Kuhn, Imre Lakatos believes that scientific theories are not strongly falsifiable. In Lakatos's [44] Methodology of Scientific Research Programmes (MSRP) evidence counts against a theory only when there exists a rival theory which not only explains the success of the other theory, but is able better to account for the contrary evidence. In MSRP the immunity from revision of scientific theories corresponds to Kuhn's claim that in normal science anomalies never count against a paradigm, but merely against the ingenuity of scientists in applying it. The necessity of a rival theory for disconfirmation to occur corresponds to Kuhn's idea that anomalies are a threat to the ruling paradigm only once alternative conceptual schemes, which can explain the anomalies, become available. However, Lakatos rejects Kuhn's view that "irrational" socio-political forces dictate the development of science. In MSRP this development is driven by rational heuristics, which are theory-independent methodological principles that prescribe how theories are to be compared with the evidence and with each other.

The idea that scientific theories "compete" against one another, and not against the evidence simpliciter, is also a feature of test-theory methodology, though the way in which theories compete in test-theory methodology clearly is very different from the way they compete in MSRP. In test-theory methodology rival physical theories are represented parametrically within a single conceptual framework. Competition, if one can call it that, merely involves the evidence selecting a theory over against its rivals. In this process the rival theories are definitively ruled out, or falsified, by the evidence. In MSRP rival theories are constitutive of independent research programmes (RPs). Competition between RPs, and thus between their constitutive theories, involves a race to account hypothetico-deductively for new facts without making ad hoc assumptions. Since it is always possible (supposedly) for an RP to account for any phenomenon by suitable adjustment of the auxiliary assumptions in the its protective belt, crucial falisfying experiments are never possible in MSRP. Evidence can lead to the degeneration of an RP, but never to its outright rejection.

In general terms, test-theory methodology characterises the rivalry between competing theories in a purely formal way, whereas MSRP characterises this rivalry in a partly formal, and partly historical way. The formal part involves the hypothetico-deductive prediction by individual RPs of novel facts. The historical part involves how well competing RPs manage to do this relative to one another. Since RPs, rather than their constitutive theories or laws, are the subjects of empirical assessment, MSRP supports a holist account of theory-evidence relations. Accordingly, MSRP fails to establish relevance relations between theory and evidence. Test theories, in contrast, enable evidence to be brought directly to bear on elements of theory which Lakatos would regard as constitutive of the hard core of an RP. Hence, test-theory methodology undermines Lakatos' claim that these elements are immune from revision. Consequently, test-theory methodology undermines the usefulness of Lakatos's concept of 'hard core'. Since hard cores are defining of RPs, test-theory methodology undermines Lakatos' division of physics into a set of mutually exclusive RPs.

Whereas Lakatos regards scientific theory to be only indirectly conditionable by experience, through the success of positive heuristics, test theories make possible a much more direct empirical conditioning. In so doing, test theories make sense, in formal deductive terms, of how well a physical theory will do over time. If a physical theory has already been richly overdetermined by the test theory method, then very likely it will succeed eminently well in predicting novel facts. But if a theory has been falsified by the test theory method, then it will fail even to account for already known facts. In this way, test-theory methodology makes sense, in purely formal terms, not only of the historical success of a physical theory, but also of the historical failure of its rivals.

5.8 Test theories and bootstrap methodology

Clark Glymour's "bootstrap" account of confirmation conforms more closely to test-theory methodology than perhaps any other philosophical account of confirmation, including the reductionist account. The primary motivation for bootstrap methodology was Glymour's conviction that holist tendencies in the philosophy of science simply could not make sense of the "delicacy and ingenuity with which scientific practitioners attempt to establish the relevance of some bit of evidence to some bit of theory".[16] On this basis, Glymour has criticised not only formalist accounts of confirmation, such as those advanced by Hempel and Popper, but also historicist accounts, due to Kuhn and Lakatos. The failure of all these accounts to establish relevance relations between theory and evidence Glymour diagnoses as being due to their explicit or latent hypothetico-deductivism.

Inspired by reductionist methodology, Glymour aimed to remedy this failure with his bootstrap account of confirmation. In general terms, boot-

[16] Glymour [31], p. 4.

strap methodology specifies how evidence E may generate instances of a theoretical proposition H by means of background theory T. More specifically, Glymour's *bootstrap condition* shows how the value of theoretical quantities $\{P_i\}$ may be computed uniquely from evidence E by combining E with T.[17] The $\{P_i\}$ are constitutive of H, and computing their values allows one to determine whether or not H is confirmed by E. T links E and H by including propositions which contain both the $\{P_i\}$ and predicates of the observation statements E. On the face of it, the propositions in T are just the analytic reduction sentences of Carnap and Reichenbach. However, Glymour insists that these propositions are not necessarily analytic: "the link between the vocabulary of a hypothesis to be tested and the vocabulary of the evidence is not provided by a special class of analytic hypotheses, but may be provided by any hypothesis whatsoever".[18] Glymour explains, for example, how Newton used his Second Law of Motion to show that the accelerations described by Kepler's Laws implied the existence of inverse-square forces, "but surely Newton's second law is no analytic truth; it is not even a truth".[19]

Glymour's bootstrap methodology shares with test-theory methodology a non-hypothetic-deductive, anti-holistic, but positive conception of the relation between theory and evidence. For both methodologies seek to confirm specific theoretical hypotheses, or propositions, by deriving instances of them from phenomena. Nevertheless, bootstrap methodology is in an important sense more general than test-theory methodology. Whereas test theories have been developed by scientists who have in mind the empirical conditioning of mathematical laws, bootstrap methodology applies in settings "in which the hypothesis is not necessarily an equation but any sentence at all, and the theory with respect to which the hypothesis is tested is likewise unrestricted".[20] Indeed, Glymour's characterisation of Newton's test-theory argument as an example of the bootstrap method suggests that test-theory methodology is merely a special case of Glymour's general theory of confirmation.

Yet both Newton, as William Harper [38] shows, and modern physicists, as I show, have, by means of test-theory constructions, set up relationships between theory and phenomena, which are at once stronger and more systematic than Glymour's bootstrap relations.[21] Test-theory relations are stronger than bootstrap relations because they are biconditional relations. Whereas, both bootstrap and test-theory methodology deduce elements of theory from observations, test-theory methodology, in addition, deduces these same observations from the theoretical elements. Newton, for example, established a biconditional relationship between an inverse-square

[17]Glymour [31], pp. 130–131.
[18]Glymour [31], p. 150.
[19]Glymour [31], p. 150.
[20]Glymour [31], p. 123.
[21]Harper's article is not about test-theory methodology as such, but about general features of Newton's deductions from phenomena.

force law and Kepler's Third Law. The significance of biconditionality is that for a given phenomenon only one hypothesis is compatible with it, and conversely. Thus, biconditionality signifies a very tight connection between theory and evidence.

Test-theory relations are more systematic than bootstrap relations because they obtain not merely between a specific phenomenon and a specific element of theory, but systematically over a whole range of phenomena and theoretical elements. As Harper has pointed out such systematic equivalences "make the phenomenon measure the value of the theoretical magnitude [i.e. test-theory parameter] specified in the proposition inferred from it". [22] Indeed, this kind of systematicity between theory and observation is a defining feature of test-theory constructions.

Thus, while bootstrap methodology goes beyond test-theory methodology, by being more generally applicable, test-theory methodology in turn goes beyond bootstrap methodology, by being able to establish tighter, more systematic connections between theory and phenomena. In my view, the systematic character of test-theory relations can actually help sharpen Glymour's criticism of what he calls "the new fuzziness", i.e. the attempt to make sense of science primarily in historical and sociological, rather than in formal, terms. Glymour includes in the new fuzziness Kuhn's and Lakatos's accounts of science. He is particularly critical of MSRP, but not as critical as he might be given test-theory methodology. For bootstrap methodology does not directly challenge the division of science by MSRP into a set of mutually exclusive RPs. By systematically representing rival physical theories within a single formal structure, however, test-theory methodology does directly challenge this division, and thus, in my view, provides a stronger criticism of MSRP than is possible given bootstrap methodology.

Test-theory methodology in fact combines, in a formal way, the two senses in which confirmation is a three-way, rather than a two-way, relation. Test-theory methodology shares with bootstrap methodology the idea that theory cannot be derived from, and thus confirmed, by phenomena, without employing theoretical background assumptions. Test-theory methodology shares with MSRP the idea that confirmation is not merely a two-way fight between theory and evidence, but a three-way fight involving competition between rival theories. Indeed, this last feature of test-theory methodology—its systematic character—makes it more formal, and less fuzzy, than even bootstrap methodology.

Despite their differences, the bootstrap and test-theory accounts of confirmation are similar enough to share a number of virtues. An especially striking virtue—one which is not shared by hypothetico-deductivism—is that both accounts allow us to make sense of the methodological truism that theory is better supported by a variety of evidence than by a more homogeneous body of evidence. In test-theory methodology there are cases where different phenomena determine different test-theory parameters and

[22] Harper [38], p. 186.

thus determine different parts of physical theory. In these cases a variety of evidence allows more of our theory to be empirically determined. Then there are cases where different phenomena determine the same test-theory parameter and thus overdetermine a specific part of our theory. In these cases a variety of evidence allows for an especially strong determination of one part of our theory.

There is one very important, but limiting, feature of both test-theory methodology and bootstrap methodology, which is not explicitly acknowledged by Glymour. The bootstrap account of confirmation was motivated by the failure of post-reductionist accounts to establish relevance relations between theory and evidence. But neither the bootstrap account nor the test-theory account overcomes this failure entirely, because neither account tells us which background assumption to impugn when test-theory or bootstrap arguments go awry. I think the reason that Glymour does not acknowledge this limiting feature of his account is because he has too idealistic a conception of the bootstrap relation.

To see this, consider Newton's derivations of the inverse-square character of the Sun-centred force from phenomena. (See Section 2.4.) These derivations require not one but several pieces of background theory T to be cogent derivations. They require not only Newton's Second Law of Motion, but also, for example, propositions specifying the shape of planetary orbits. However, Glymour implies, misleadingly, that just one proposition from T (Newton's Second Law) will suffice to link phenomena from E to the inverse-square relation H. Given that several propositions from T are in fact required, it is clear why even bootstrap methodology does not overcome entirely the problem of evidential relevance. For in the case that disparate pieces of evidence yield different values for the same theoretical quantity, there is no formal procedure for deciding which of our theoretical propositions from T is impugned.

The point at which test-theory and bootstrap arguments meet limits, then, is the point at which general historical and less-formal methodological considerations, of the sort that Kuhn and Lakatos lay before us, must take over. In contrast to Glymour, I acknowledge that there are historical dimensions to confirmation and meaning in physics, though it is clear, by the lights of the bootstrap and test-theory accounts, that these dimensions are far more limited in their extent than some philosophers of science would suppose.

5.9 Are test-theory arguments eliminative inductions?

The methods of demonstrative and eliminative induction may be regarded as attempts to modify simple enumerative induction, in order to make inductive inference acceptable to deductivists. Whereas demonstrative induction is a positive account of theory confirmation, eliminative induction is a more critical account. In this section I will clarify the relationship of both accounts to bootstrap and test-theory methodology.

Enumerative induction involves the inferring of universal (theoretical) statements from singular (phenomenal) statements.[23] The basic strategy of demonstrative induction is to make this inference deductively valid by adding further premises, of a general character, to the argument. Typically, the additional premises specify hypotheses which are constitutive of some theory or other. The merit of this strategy is not merely its potential to appease deductivists. More important is the "relocation" of inductive risk in the premises of the argument, "where its import, nature, and magnitude can be assessed far more readily".[24] As John Norton points out, "the flight to demonstrative induction does not and cannot free us of the need to employ ampliative inferences", for the simple reason that enumerative induction ultimately will be needed to justify the additional premises. However, the replacement of "rule-bound" with "assumption-bound" inductive risk is a move which makes our assessment of the risk involved far more tractable. Specifically, the degree of risk corresponds to how innocuous are the additional general premises required to turn an enumerative into a demonstrative induction.

In approach, eliminative induction is similar to demonstrative induction. The basic strategy of eliminative induction is, once again, to make enumerative induction deductively valid by adding further premises. This time, however, the premises specify not the hypotheses of a single theory but a universe of possible theories or hypotheses. In the ideal case, the premises containing singular (phenomenal) statements act to eliminate, or falsify, all but one of the rival theories. Like demonstrative induction, eliminative induction shifts the inductive risk from the inference rule to the premises of an argument. In eliminative induction the degree of risk corresponds to the size of the universe of possibilities: the more theories that are included, the less the risk.

Since both demonstrative and eliminative induction establish logical relations between theory and phenomena by specifying further statements of a theoretical nature, it would seem that these argument forms should be closely related to bootstrap and test-theory arguments. In particular the positive, but unsystematic, character of demonstrative induction suggests a close association with bootstrap inferences, whereas the more critical, systematic nature of eliminative inductive suggests a close association with test-theory inferences. Indeed, John Earman [19] identifies the application in physics of the PPN Formalism as an eminent example of eliminative induction.[25]

In my view Earman's and Norton's thesis that eliminative induction has played, and continues to play, an important role in the development of relativistic physics is in good accord with the physicists' own view that test theory constructions are valuable methodological tools for evaluating

[23]Hempel [39], Chapter 2.
[24]Norton [60], p. 14.
[25]Earman [19], pp. 173–180.

the empirical warrant of Einstein's theories. Although the terminology of philosophers and physicists differ here, both groups appear to be in substantial agreement about the way in which theory-evidence relations are established in relativistic physics. In particular, both philosophers and physicists stress how competing relativistic theories may be compared systematically and uniformly with phenomena in a way which places tight bounds on the class of viable theories. Such consensus between philosophers and scientists on methodology is a rarity, and makes this area of research an especially revealing one. It indicates, in my view, a degree of a reflectiveness about method, on the part of physicists, which is not often ascribed by philosophers to practising scientists. It also indicates a degree of sensitivity about the practice of science, on the part of philosophers, which is not often ascribed by scientists to philosophers. In light of this consensus I would be willing to affirm that test theory arguments are eliminative inductions, were it not for some fine-grained distinctions between the two methodologies which I will now describe.

To date, the term 'test theory' has been applied by physicists only to cases in which there exist continuously parameterised classes of theories. In contrast, 'eliminative induction' traditionally has been applied by philosophers to cases involving rag-bag assortments of theories. A telling criticism of eliminative induction in the past has been that in such cases one cannot be sure that all possible theories are represented in the premises of the argument. The continuous parameterisation of test theories, however, ensures that within a well-defined class all possible theories are included. Earman and Norton are well aware of past criticisms of eliminative induction and take care to point out that they regard what I have called test-theory arguments as especially impressive examples of eliminative induction. One advantage, then, of the physicists' terminology is that it does not carry with it this regretable connotation carried by eliminative induction concerning the completeness of the theories being tested. Another advantage is that the test-theory idea, unlike eliminative induction, draws attention to the fact that evidence may strongly rule out or strongly confirm physical theories, by having the values of test theory parameters be richly overdetermined by observations.

Perhaps the most important difference between test-theory arguments and enumerative induction concerns their structure. While the conclusions of inductive arguments always contain theoretical propositions of a universal character, the conclusions of test-theory arguments never do. At most, test-theory inferences yield conclusions which are instances of universal propositions, but they never yield universal propositions themselves. For example, test-theory arguments involving the PPN Formalism deliver metrics which describe the spacetime structure in the vicinity of specific material bodies, but they do not deliver metrics which are applicable to all bodies. The specific conclusions of test-theory arguments may well provide inductive support for more general propositions. Nevertheless, the arguments themselves are not in any way inductive arguments. Test-theory arguments are concerned directly with determining the properties of specific physical systems, and only indirectly with determining the scope of these properties, by providing the necessary premises for inductive arguments.

The case of Newton and universal gravitation provides a good illustration of why it is a mistake to construe some test-theory arguments as eliminative inductions. Newton, recall, carefully distinguished between the deductive and inductive aspects of his methodology. According to his experimental philosophy the properties of bodies should first be deduced from phenomena and then, as far as possible, rendered general by induction. Newton regarded the deduction of physical properties is prior to, and necessary for, the possibility of inductive generalisations. (See Section 2.5.) In *Principia* Newton employed his test-theory construction to deduce the inverse-square character of various forces acting in the Solar System. While his test-theory argument certainly ruled out universal forces of a character other than inverse-square, Newton did not conclude from this negative fact, as an eliminative inductivist might, that there exists a universal inverse-square force.

Such an approach Newton would have regarded as at once too critical and too speculative. Newton would have regarded this approach as too critical because he was against deducing properties "only from a confutation of contrary suppositions". Rather, his method was to derive them "positively and directly" from experiments.[26] Newton would have regarded this approach as too speculative because it assumes the existence of some universal force, whether inverse-square or otherwise, and in the early stages of his argument, where his test theory is employed, Newton had not yet amassed sufficient evidence to argue cogently that the inverse-square forces acting in the Solar System act between all material bodies.

The "inductive part" of Newton's argument, by which he establishes the existence of a universal force, is in fact subtle and ingenious—too much so, I believe, to be captured by any simple inductive scheme. It involves the cautious employment not only of his inductive Rules of Reasoning, but also other methodological rules concerning simplicity, as well as further deductive arguments which establish that celestial forces are generated by the mass of celestial bodies.

One difference, then, between eliminative inductions and test-theory arguments is that the former, unlike the latter, presuppose the existence of specific universal laws. For this reason, eliminative inductions can really only be performed when there already is a high degree of confidence in the existence of such laws. Test-theory arguments, on the other hand, are not bound by this stricture, because their conclusions, though theoretical, are singular rather than universal in character. Earman can get away with characterising as eliminative inductions physicists' application of test theories for relativistic gravity, because Newton has already established for us the universality

[26]Letter to Oldenburg, dated July 1672. Cited by Thayer [76] on p. 7 of his book.

of gravity. What Earman could not do is construe Newton's test-theory arguments in the same way. Hence, in some important cases it would be misleading to construe test-theory arguments as eliminative inductions.

5.10 The empirical basis of test-theory methodology

This section is somewhat different from previous ones. In previous sections I compared the test-theory method with philosophical accounts of confirmation, in order to gain a better understanding of the strengths and limits of the method. In this section I am still interested in the method's strengths and limits, but not by way of comparing the method with other methods of confirmation. Rather, in this section I ask what ultimately is the justification for the test-theory method. Why should we believe in this method? Why should we accept the conclusions of test-theory arguments?

A traditional philosophical approach to answering these questions would seek to ground test-theory methodology metaphysically. It would seek to demonstrate the workability of the test-theory method by showing that the method conforms in some appropriate way to the world as it really is. This metaphysical approach is not the one I will take in the present section. My approach is in fact the exact opposite of the metaphysical approach. Instead of asking whether the method should work, I argue empirically (from the history of physics) that it does work. I then ask what the (phenomenal) world must be like given that the method works.

To answer the question "Does the test-theory method work?", we need to know what it is for a method to work, and that depends on the aims of science. The primary aim of science has always been to explain natural phenomena. To a greater extent than any other field of inquiry, physics has been successful in explaining phenomena. The explanatory power of physics resides in its theories. The power of these theories is due to their ability not merely to explain this or that phenomenon, but to explain, and thereby unify, a vast range of seemingly disparate phenomena. Thus, to answer the question "Does test-theory methodology work?" we need first to answer the question "Does test-theory methodology warrant physical theories which possess the kind of explanatory and unifying power that we seek?" I submit that it does, and eminently so. For, in Chapters 2 and 3 I showed how test-theory constructions have been used by Newton and by contemporary physicists strongly to warrant some of the most explanatory and unifying of our physical theories. In my view, the history of physics provides excellent reason for believing that test-theory methodology works.

I want to emphasise that the kind of empirical, or pragmatic, justification I am here affording the test-theory method is necessary for our accepting the method, and that logical analysis alone will not provide sufficient warrant. One might think that because test-theory methodology confirms theory by deriving theory from phenomena, it is a fortiori stronger than, say, hypothetico-deductivism, which confirms theory by deriving phenomena from theory. Test-theory confirmation seems simply to possess greater logical force than hypothetico-deductive confirmation. However, suppose that the (phenomenal) world were a rather unsystematic place. Suppose, for example, that the motion of a planet's apsides were entirely unconnected with other characteristics of its orbit. These phenomena would yield different values of the test-theory parameter in Newton's test theory. In a thoroughly unsystematic world such failures would be endemic to test-theory applications. The test-theory method would not work, despite its impeccable logical credentials.

This result reveals something deep about the test-theory method. The method presupposes that the world is a system, that is, an entity whose individual parts are connected to one another, and connected not in a haphazard but in an orderly way. To the extent that test-theory methodology works we can say that the world is a systematic place. There is no guarantee, of course, that test-theory methodology will continue to work indefinitely. There may come a time when this methodology fails us on a regular basis, in which case we would have to conclude that there is a limit to the extent to which the world is systematic. Until such a time, however, the past success of test-theory methodology urges us to continue using it.

The argument for the systematicity of nature I have just presented has been advanced already by the Philip Catton [12], not on the basis of test-theory methodology, but on the basis of bootstrap methodology, which, as we saw in Section 5.8, is a very closely related account of confirmation. Catton argues that the success of bootstrap inferences in delivering fundamental theories of great explanatory and unifying power provides good evidence for believing that nature is a system. Catton's prime examples of application of the bootstrap method are Newton's derivation of his inverse-square law, Maxwell's derivation of the field equations of electromagnetism, and Einstein's derivation of the Lorentz transformation. My study of modern test theories for relativistic kinematics and gravitation provides further support for Catton's thesis that nature is, at its deepest levels, systematic in character.

Catton uses his result to criticise the "new experimentalist" view of nature expounded by the philosopher of science Nancy Cartwright [10, 11]. Like Catton and myself, Cartwright believes that the question of whether or not nature is a system is quite definitely an empirical, rather than a metaphysical, question. She believes that the question can be answered by studying the practice of science, and she believes that methods employed in that practice indicate to us what the answer is. "We learn what the world is like by seeing what methods work to study it ... "[27] However, unlike Catton and myself, Cartwright concludes from her study of the methods of physics that nature, at its deepest levels, is unsystematic.

The basis for Cartwright's view is her belief that the fundamental theories of physics, while unifying, are not true of the world, not even approxi-

[27] Cartwright [11], p. 1.

mately so. The reason why she thinks that fundamental theories "lie" about the world is her belief that the kind of methods which establish connections between such theories and the phenomena are not truth-preserving. While Cartwright accepts that bootstrap and test-theory-type inferences are used to established low-level phenomenological laws (which while true do not explain much) she seems to be unaware that precisely the same kind of inferences can be used to support high-level fundamental laws. This at least is one important criticism that Catton makes of Cartwright's work.

Cartwright develops what she calls a "simulacrum" account of theoretical explanation, by which fundamental theories are brought into connection with the phenomena.[28] According to this account the act of explaining is a two-stage process. In the first stage fundamental laws are used to construct idealised models of the physical system at hand. The fundamental laws are true of these models, but the models are not true of the phenomena. In the second stage these models are adapted to specific phenomena by adding to them such terms as are necessary to account for all the "causally relevant characteristics" of the system. A phenomenon is counted explained once it has been derived from an appropriately amended model. According to Cartwright, the modification of idealised models required to make them "fit" real physical systems indicates the falsity of our fundamental laws.

I believe that Cartwright is right to stress the importance of models in scientific explanation, but wrong in believing that idealised models cannot provide a truth-preserving "bridge" between phenomena and fundamental theory. Take the PPN Formalism, for example. The central equation of the PPN Formalism, Equation 3.18, is a generalised spacetime metric which incorporates a number of metrics from rival theories of relativistic gravity. Each of the metrics so incorporated does not itself, strictly speaking, constitute a physical law, for the laws of relativistic gravity theories are differential equations, not metrics. Rather, the metrics represented in the PPN Formalism are all models of their associated theories—they are solutions of their theories' differential equations. Moreover, they are idealised models, because they presuppose that gravitating matter is representable by a perfect fluid and that there is zero gravity (i.e. no matter) at spatial infinity. Yet time and again physicists have been able not merely to determine but to overdetermine the PPN variables which parameterise specific features of these models. Since relativistic gravity theories are true of these models, test-theory arguments in the PPN Formalism give us good reason, I think, to believe that the theories are true of the world.

The reason idealised models can provide a truth-preserving bridge between fundamental theory and phenomena in the context of test-theory methodology is due to the fact that test theories are able to bring phenomena to bear on very specific parts of these models. So long as we direct the focus of our test theories onto theoretical elements that are shared by

[28]Cartwright presents her simulacrum account in Essay 8 if her book "How the Laws of Physics Lie".

both our idealised and more-realistic models, then no problems will arise in using idealised models. For, our fundamental laws are true of these elements, irrespective of what kind of model they appear in.

The empiricist, or naturalist, line on method I have run in this section, and used to justify test-theory methodology, is in one important way similar, but in another important way different, from the more skeptical approach to method taken by Paul Feyerabend [27]. Feyerabend has emphasised the rich diversity of science in both its subject matter and methods. He has opposed, as I do, the way in which some philosophers have sought to impose a priori constraints on method, and prescribe to scientists, on that basis, the one and only true method. I certainly do not wish to make such prescriptions with regard to test-theory methodology. The test-theory method has been developed by physicists for the purpose of confirming physical theories. Practitioners of test-theory methodology make no suggestions that the method should be generally applicable to all science. Indeed, the mathematical character of test-theory constructions precludes this possibility. Yet the success of the test-theory method surely provides physicists with good reason to continue using the method, both to confirm current physical theory and as a tool for further theorising. The down side to Feyerabend's pluralistic view is that it seems blind to the fact that in the practice of science some methods can prove to be more successful than others. The test-theory method is an important example of a scientific method which has proved fruitful in the past and is now enjoying the favour it therefore deserves.

CHAPTER 6

Conclusion

Test theories have been devised by physicists in order to strengthen and clarify the empirical credentials of fundamental physical theory. In my view, the existence and fruitful application of the test-theory method is of great significance both for the philosophy of science and for theoretical physics. In Section 6.1 I summarise my discussion of test theories from previous chapters, and I draw conclusions about the importance of test theories for our understanding of the methods, foundations and history of physics. In Section 6.2 I suggest, on the basis of my conclusions, that the test-theory method is relevant to the unification programme in modern theoretical physics. This method indicates, I believe, what the priorities of the unification programme should be.

6.1 The philosophical significance of test theories

Test-theory methodology is significant for the philosophy of science in two respects. Firstly, it shows philosophers just how well physicists understand the methods they employ. Secondly, it has important bearing on what philosophers themselves have said about physics and its methods. I will discuss each of these two matters in turn.

Test theories have been developed for physicists by physicists. Test-theory methodology is not the result of a philosopher's attempt to bring to order various elements of the practice of physics, but represents regularities in that practice which physicists themselves have identified and made explicit. Physicists rarely make methodological features of their work so explicit. The recognition given by them to what are now called 'test theories' signifies the worth to physicists of these methodological constructions. Test theories, in fact, enable physicists to further significantly the primary aim of science, which is to provide explanations of natural phenomena. Test theories do this by delivering to physicists theories of great explanatory power.

In light of the success of test-theory methodology, the recognition that physicists give to test-theory constructions shows, in my view, just how reflective about their methods physicists really are. This reflectiveness about method is not of a traditional philosophical kind. Physicists do not argue for their test-theory method on the basis of what the world is really like.

Physicists do not seek, that is, to ground their method metaphysically, as a philosopher might. Rather, physicists expect us to accept their method solely on the basis of its results. This makes physicists empiricists about method. In my view, the success of test-theory methodology illustrates how methods of empirical science are themselves discovered empirically.

Newton was probably the first physicist to use a test theory. He was certainly the first to make profound and far-reaching use of one. Newton used his test theory to argue for the existence of inverse-square forces in the Solar System, and to argue, ultimately, for universal gravitation. He thereby provided test-theory methodology with its first notable success. Newton's own reflections on scientific practice illuminate the character of the test-theory method. Although Newton did not identify with a name his test-theory construction, he did identify his application of it as an example of his method of deduction from phenomena. As deductions from phenomena, test-theory arguments allow one to derive confirming instances of theory from observations. Since it is possible to deduce such an instance from two or more disparate-seeming phenomena, as Newton illustrated, test theories make possible an especially strong form of confirmation. For Newton, the ultimate purpose of deductions from phenomena (and hence of test theories) was to ensure that physical theories explain at least the phenomena they are derived from, and hopefully many more phenomena besides. Newton believed (and was proved correct) that deduction from phenomena (and hence test-theory methodology) would surpass rational speculation as a method for discovering theoretical explanations of phenomena.

Newton's example, and his reflections on the nature of science and the world, help us to identify general conditions which knowledge and the (phenomenal) world must satisfy if the test-theory method is to work. Specifically, our physical knowledge of the world must be mathematical, and the world itself must, to some extent, be systematic, or unified, in character. If test theories are to work, our physical knowledge of the world must be mathematical, because test theories are themselves mathematical constructions. Newton stressed the mathematical character of his dynamical conceptions over against the non-mathematical hypotheses posited by mechanical philosophers. Newton's dynamical principles constituted the conceptual presuppositions of his test theory. In the absence of such principles, Newton's test theory would not only have not worked, it would not even have been possible. If test theories are to work, the physical world itself must to some extent be systematic, or unified, in character. For if the world were not of this kind, phenomena would regularly yield conflicting values of test-theory parameters (as I pointed out in Section 5.10). Such conflict would preclude us from ever obtaining, by the test-theory method, unifying theories which explain a diverse range of phenomena. Thus, the extent to which test-theory methodology works provides a measure of the extent to which the world is systematic, or unified.

The development and fruitful application of test theories for relativity shows that Newtonian methods, and the test-theory method in particular, transcend Newtonian physics. Modern physicists have, however, extended the test-theory method, and in doing so have provided further warrant for the method. They have shown how test theories of considerably greater complexity than Newton's are possible, and they have shown how such test theories can be used to condition empirically elements of theory even more fundamental than Newtonian forces. The reflections of physicists today, on characteristic and important features of test theories, further illuminate the test-theory method. Physicists highlight, in particular, the way in which test theories (by means of their parametric structure) show explicitly just what element of physical theory is being tested by a given phenomenon. Robertson states, for example, that he uses his test theory to test "aspects of the Lorentz transformations which are insensitive to the Michelson-Morley experiment". Both Robertson and Schiff admire the way in which Eddington's test theory for relativistic gravity allows them to keep track of exactly what part of gravity theory is being conditioned by observations. Evidently, modern physicists are concerned about establishing relations of relevance between theory and phenomena, and they regard test theories as eminently suited to this task.

Physicists also highlight the fact that test theories enable them empirically to select a physical theory from out of a parameterised class of alternatives. They are impressed, in other words, with the formal and systematic way in which test theories relate physical theories to evidence. In developing the PPN Formalism Nordvedt and Will state explicitly that their aim was to "systematise the comparison between theory and experiment". The PPN Formalism is a particularly fine example of a test theory which systematises theory-evidence relations. Its rich structure enables a wide variety of competing theories of gravity to be compared uniformly with observations.

These facts about test theories, which physicists themselves draw our attention to, have important bearing on what philosophers have said about physics and its methods. Above all, test-theory methodology invites us to reassess the contribution made by logical positivists to our understanding of science. The positivists demanded that theoretical meaning be made out solely in empirical terms, and they demanded (originally) formal clarity of the meaning relation. Most philosophers today have little sympathy for these demands of the positivists. The failure of the reductionist programme, in particular, has encouraged the development of accounts of science which require neither that theory possess purely empirical meaning nor that theory-evidence relations possess formal clarity. Yet, in developing test theories physicists seem to be striving towards, and indeed to have fulfilled in large measure, the ideals of reductionism. For test theories establish formal, systematic relations between theory and evidence. They show how particular phenomena bear on, and are thus able empirically to condition, particular elements of physical theory. In so doing, test theories clarify the empirical meaning of those theoretical elements. In my view, test-theory methodology in physics constitutes a concrete and surprisingly full realisation of positivist aspirations for science. That this realisation has obtained in physics (rather than in some other science) is perhaps no surprise. It was, after all, developments within physics which originally inspired the positivist programme.

Those features of test-theory methodology which support the reductionist model, accordingly undermine other philosophical accounts of confirmation. The much-vaunted hypothetico-deductivist account, which many positivists later endorsed (mistakenly, I think), ascribes to scientific theory a grossly speculative character. In physics, however, the speculativeness of theories is in fact quite limited, because test-theory methodology enables elements of theory, including some foundational elements, to be empirically determined. Historicist accounts of confirmation, like Kuhn's and Lakatos's, ascribe to scientific theory a holistic character, and construe rival theories as incommensurable belief systems that cannot be definitively ruled out by contrary evidence. In so far as these accounts apply to physics, however, they are of strictly limited worth. For test-theory methodology shows how evidence can condition very specific parts of physical theory, how rival theories can be represented parametrically within the same formal structure and compared in a uniform way with the evidence, and how crucial falsifying experiments are possible.

Test-theory methodology shows, in particular, how Galilean and Einsteinian kinematics may be compared in this way with the evidence, and indeed how Galilean kinematics has been definitively ruled out, or falsified, by this evidence. That test theories are possible in both Newtonian and Einsteinian physics testifies to the unity that exists at the level of test-theory methodology across seemingly disparate historical phases of scientific inquiry. That fundamental but conflicting elements of Newtonian and Einsteinian physics can be incorporated within a single test-theory construction testifies to the great commensurability of Newtonian to Einsteinian physics. In my view, philosophers of science need to reassess, in light of test-theory methodology, just how different classical and modern physics really are.

6.2 Test theories and the unification programme in physics

The name 'test theory' implies that physicists today use test theories primarily as tools for subjecting already existing physical theories to empirical scrutiny. But there is another, more significant, use of test theories. The very first use of a test theory, by Newton, showed how test theories can be used as tools for theory discovery. In this section I argue that the test-theory method is the method physicists today need in order to discover a more unified theory of the fundamental interactions.

In Section 3.9 I pointed out how modern test theories enable physicists to establish the conditions necessary for further unification in physics. Here, I will argue that the test theories should actually be instrumental in forging that unification. To establish that the test-theory method is relevant in this way to the unification programme, I reexamine how a test theory helped Newton to discover a unified theory of celestial and terrestrial motion. I consider first the ways in which Newton's test theory contributed to the unifying character of his gravity theory. Then, I discuss the epistemological conditions necessary for Newton's test-theory unification. Finally, I ask whether these conditions obtain in modern theoretical physics.

Newton always understood his methodology as forward-looking. The whole point of experimental philosophy was not to confirm already existing physical theories—theories which Newton found woefully inadequate—but to discover new and better theories, to succeed, that is, where the speculative method of rational mechanical philosophy had failed. In *Principia* Newton provided a spectacular demonstration that his methodology could indeed deliver what mechanical philosophers had always sought, but which their methods had precluded them from ever obtaining: simple physical principles capable of explaining, and thereby unifying, a multitude of diverse phenomena. The simple, unifying physical principle that Newton discovered in *Principia* was, of course, the law of universal gravitation.

Crucial to Newton's discovery of this law was his use of a test theory. There are in fact two ways in which Newton's test theory contributed to the law of gravity's unifying character. Firstly, this test theory showed how two very different features of planetary orbits (period-radius ratio, and quiescent apsides) could condition identically the same element of theory (distance dependence of a force). Newton's test theory thereby guaranteed that an inverse-square law of force would explain, and thereby unify, both features of planetary orbits. Secondly, Newton's test-theory results made plausible the application of his Rules of Reasoning, which are the unifying methodological principles Newton used to extend the scope of his inverse-square law to all bodies.

The epistemological conditions necessary for Newton's test-theory unification are empirical and theoretical in character. The empirical basis of Newton's application of his test theory is constituted by his Phenomena. Newton's Phenomena are relationships obtaining between observable physical quantities which specify the character of various planetary and lunar orbits. Key features of Newton's Phenomena are (1) that they are mathematical in character, which means that they are of the right form to act has premises in a test-theory argument, and (2) that they are robust, and so can be trusted to condition physical theory which will later be used in explanations.

The conceptual basis of Newton's test theory is constituted by his dynamical principles, i.e. his Laws of Motion. In so far as theoretical unification is concerned, the important feature of these principles is their generality. They apply, or are intended to apply, to all motion, not just to some specific kind of motion (e.g. terrestrial or planetary). That Newton could use his test theory to unify phenomena depended crucially on the generality of its conceptual presuppositions.

Physicists today identify four basic ways in which pieces of matter can interact with other pieces of matter: by gravity, by electromagnetism, and by the strong and weak nuclear forces. These four interactions are described by physical theories which are profoundly unifying of phenomena. Yet, physicists are now searching for theories which will explain, and thereby unify, the four fundamental interactions. Ideally, physicists want just one theory which explains all the interactions. In my view, the method physicists need to discover a unified theory of the fundamental interactions is just the method that are already using to confirm lower-level physical theories, namely, the test-theory method.

The case of Newton shows that the test-theory method can be used to discover physical theory of a profoundly unifying sort. The possibility of test theories for fundamental physical quantities other than Newtonian forces shows that the test-theory method is not confined to Newtonian physics, but is of broader scope. The development and fruitful application of test theories for Newtonian forces, and for relativity theory, constitutes empirical warrant both for the method and for the doctrine that the world is systematic, or unified. In my view, these facts about test-theory methodology make it relevant to the unification programme in modern theoretical physics. But do the necessary epistemological conditions obtain today, for physicists seriously to consider using the test-theory method in this programme? For these conditions to obtain, there must exist both a robust empirical basis and a suitable conceptual basis for test-theory unification.

The mathematical laws which constitute current fundamental theories of physics very likely constitute an appropriate empirical basis for a test-theory unification of the fundamental interactions. Most physicists regard these laws as robust, and (for that reason) as clues to be heeded in further theorising. The robustness of general relativity theory is not in doubt, for many parts of this theory have been strongly confirmed via the test-theory method. Although I have (in Section 4.8) questioned whether quantum theories of matter are similarly well-confirmed, these theories have certainly passed with flying colours a multitude of hypothetico-deductive tests. What is really doubtful is the existence of a coherent dynamical framework of sufficient generality to encompass theories as disparate as general relativity theory and the quantum field theories of matter. In all likelihood, such a framework does not exist today.

By the lights of test-theory methodology, then, the most urgent requirement of the unification programme is that a suitable dynamical framework be found which can support test-theory arguments to more unified theories. I pointed out in Section 4.9 that the way in which physicists in the past have settled upon such frameworks is by metaphysical investigation of the conceptual foundations of physics, rather than by more straightforward empirical means. Thus, while the test-theory method is quite plainly an empirical method, its relevance to the modern unification programme suggests that philosophical, rather than further empirical, investigation is needed to advance substantially the cause of theoretical physics. What is more, straightforward dimensional arguments suggest that current particle-accelerator technology falls way short of being able to probe energy levels at which phenomenal clues (i.e. quantum gravity effects) to a more unified theory should appear. For these reasons, I think that further intensive philosophising about the conceptual foundations of physics is more likely to bear fruit than is further experimental probing of high-energy phenomena. Indeed, I believe that physics today needs better philosophy more than it needs bigger particle accelerators.

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